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Thomas S. Szasz

Psychotherapy and Medical Responsibility
Joel S. Handler

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H. Aronson, A. B. Silverstein, and G. D. Klee

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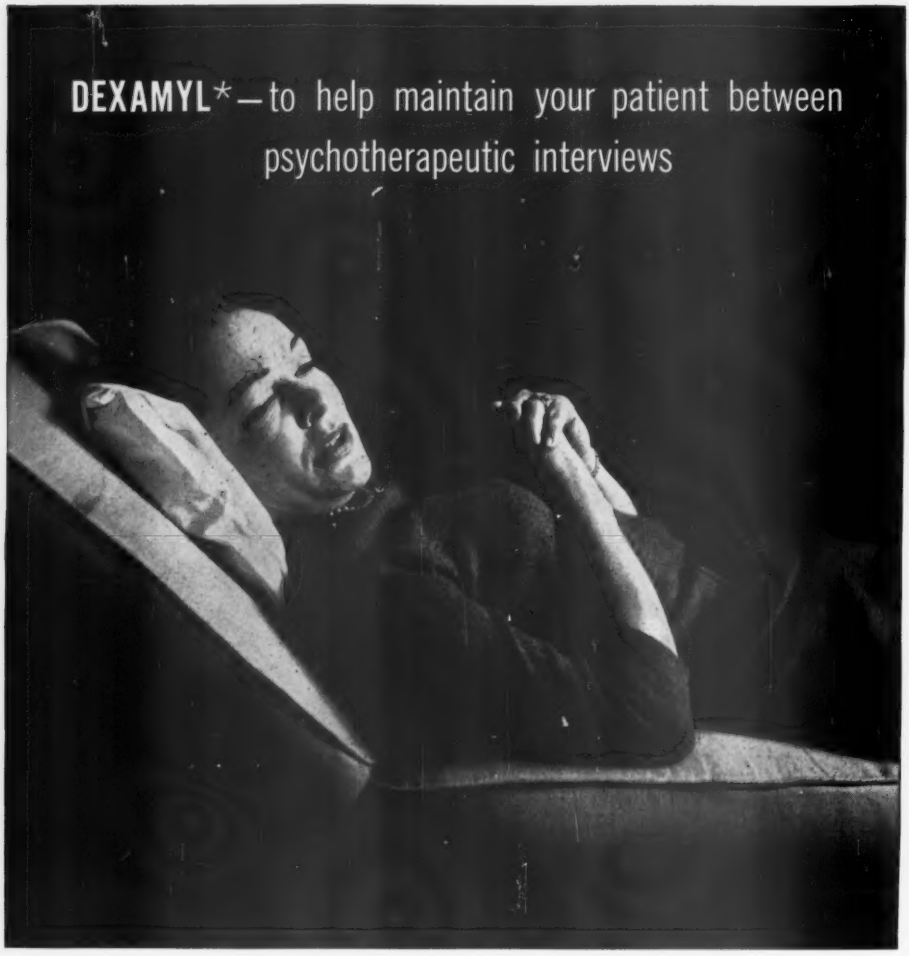

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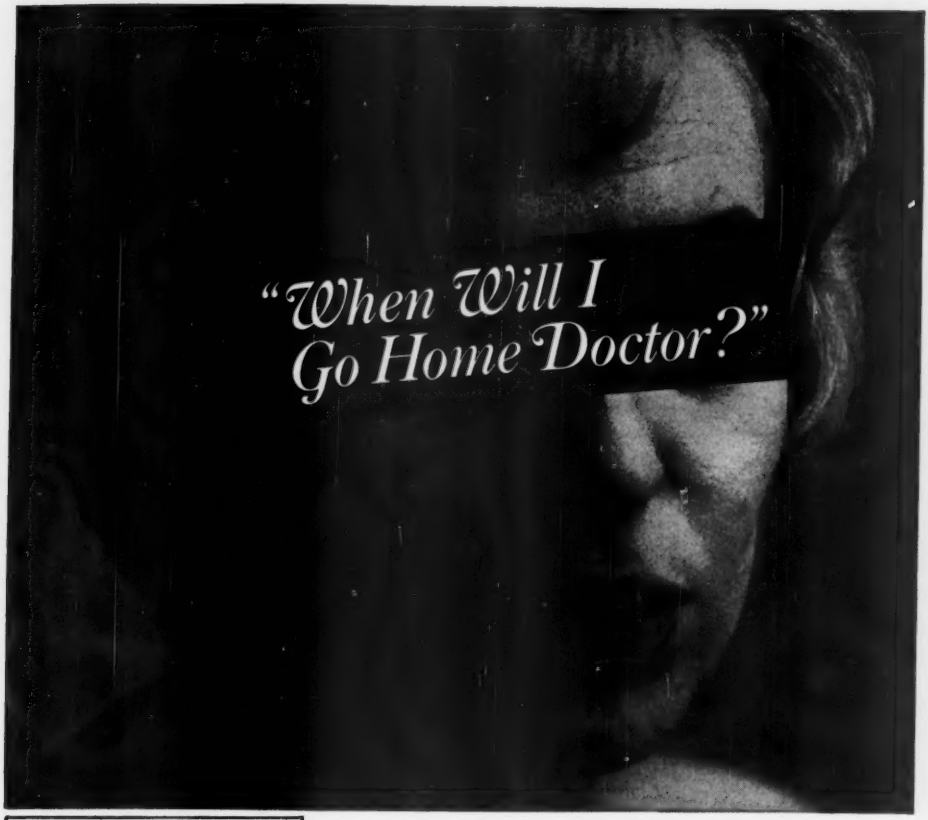

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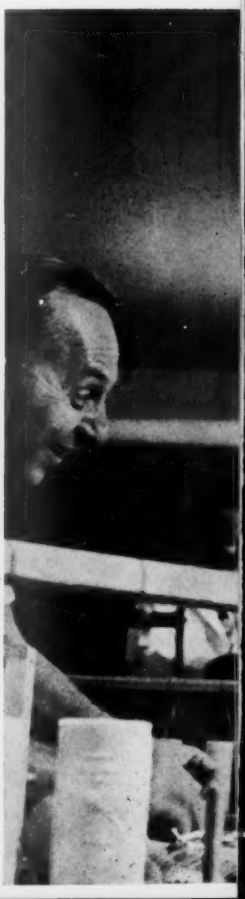

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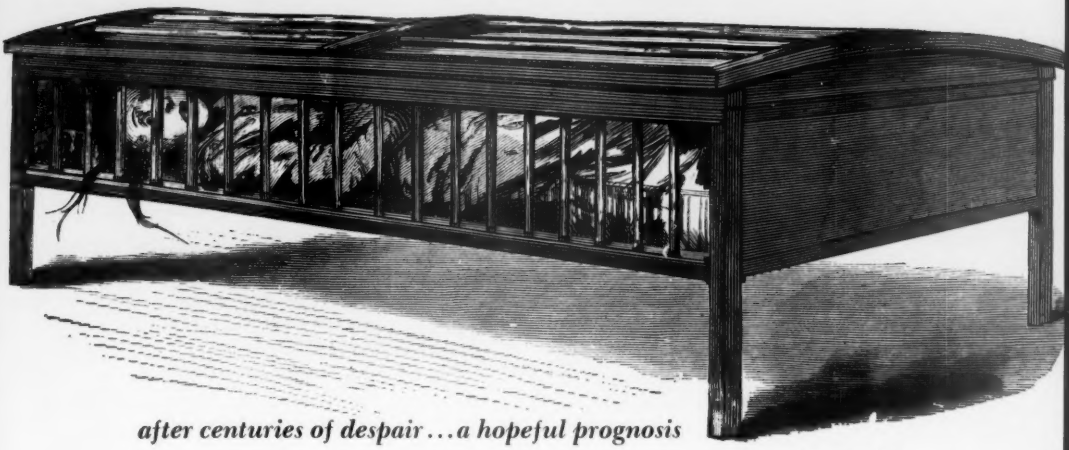

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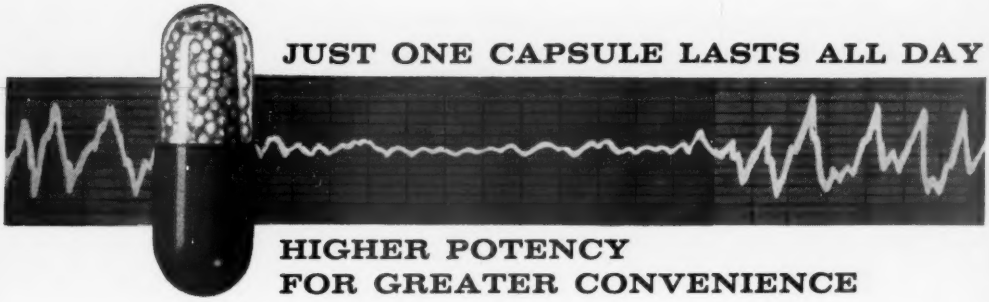

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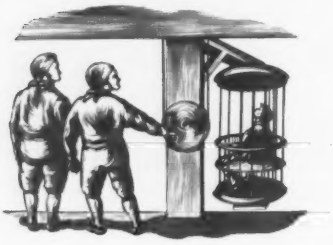

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Psychiatry, Psychotherapy, and Psychology

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During the past several years, the American Psychiatric Association has periodically released statements concerning the relationship of psychiatry and (medical) psychotherapy to psychology.[8,10] These statements embody the official position of this Association, as well as that of the American Psychoanalytic Association. The most recent of these statements, which appeared in the December, 1958, "Mail Pouch" of the American Psychiatric Association, has prompted me to offer the following comments.

Before presenting the problem and my arguments, it is essential that I state the frame of reference, or point of view, from which I shall approach this subject. I propose to regard the various "Resolutions" (which are essentially identical in content) as though they were legislative resolutions, or judicial opinions, such as the Supreme Court might render. Hence, psychiatry's official position vis-à-vis nonmedical psychotherapy is basically an attitude of judicial *prohibitionism*. Consider, for example, the Eighteenth Amendment to the Constitution, which prohibited the nonmedical use of alcohol. In a democratic society, the majority of the voters have the right to *make such a law*. This law, then, may or may not be seriously enforced; or the law may be unenforceable. As we know, in the case of drinking alcoholic beverages, the law was only partially enforced. Finally, the *decision* to enact prohibition, or to repeal it, is a social (and judicial) act. It must not be confused with scientific opinions and statements concerning the toxicity of alcohol. More will be said about this later. What I wish to emphasize now is that since social and legal decisions codify, as a rule, actions taken in the face of conflicting needs, values, and opinions, it must not be thought that they represent *the only* right or reasonable course of action. This is best illustrated, perhaps, in the minority opinions which dissenting Supreme Court Justices have traditionally written in cases involving important constitutional principles. In many instances, such dissenting opinions have carried more historical weight than did the opinions of the majority.

I have commented on this legal model of arbitrating social differences because I propose to cast my discussion of the problem of the relationship of psychiatry to psychology in this framework. Let us, therefore, regard the official psychiatric position as a law enacting prohibition—in this instance, prohibiting psychologists (and others) from the nonmedical use of psychotherapy (i.e., from using psychotherapy independently of physicians).* If we re-

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* It is important to keep in mind, however, that current medical practice acts do *not* prohibit the practice of psychotherapy by nonmedical persons.[7,9] Only the practice of medicine without a license constitutes a legal offense. It is evident, therefore,

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gard this as a majority opinion, enacting a social law, and not as an "immutable law of nature," it will be useful to consider contrary opinions. Such opinions—at least from psychiatrists—have been curiously lacking. The following comments concerning the relationship of psychotherapy to psychology, therefore, are offered in the spirit of a judicial dissenting opinion. Though, presumably, it is a minority opinion, I suspect that it is not without some supporters.†

Majority Opinion

The prohibition of nonmedical psychotherapy is set forth most clearly in the two statements to follow. The first is a quotation from a "Resolution on Relations of Medicine and Psychology," approved by the Board of Trustees of the American Medical Association, the Council of the American Psychiatric Association, and the Executive Council of the American Psychoanalytic Association.

> Psychiatry is the medical specialty concerned with illness that has chiefly mental symptoms. . . . The systematic application of the methods of psychological medicine to the treatment of illness, particularly as these methods involve gaining an understanding of the emotional state of the patient and aiding him to understand himself, is called psychotherapy. This special form of medical treatmant may be highly developed, but it remains simply one of the possible methods of treatment to be selected for use according to medical criteria for use when indicated. Psychotherapy is a form of medical treatment and does not form the basis for a separate profession.[8]

The second is from the "Statement on Psychology," which was distributed in the American Psychiatric Association's "Mail Pouch" in December, 1958. This statement was apparently prompted by the "1958 Annual Report" [1] of the American Psychological Association's Committee on Relations with Psychiatry.‡ Its main con-

that whether psychotherapy is (legally) defined as "counseling" or as "medical treatment" may have portentous consequences. It would appear that organized medicine is trying to introduce certain prohibitions into law. Since such legislation—even if it were enacted—may prove to be unenforceable, its effects on social living may prove to be especially undesirable. In addition to the example of our own Prohibition, one may consider in this connection the currently existing South African segregationist liquor laws. In that country, 12,000,000 Negroes are legally barred from drinking liquor, whereas 3,000,000 whites are unfettered in this regard. According to an article in *The New York Times*, "Checking the bootleg trade is a mammoth task. One out of every four convictions in South African courts—nearly 1,000 a day—is for illegal possession or supply of drink."[6] It would seem to me, therefore, that it behooves all of us in the psychiatric profession—especially those who advocate prohibitionist laws in regard to psychotherapy—carefully and responsibly to consider the potential social consequences of such legislation.

† In addition to Freud's[3] well-known position concerning the nonmedical uses of psychoanalysis, consider in this connection the opinions of Kubie[4,5] and Hollingshead and Redlich.[7]

‡ The contents of this report were adopted as official policy of the American Psychological Association, at its September, 1958, annual business meeting in Washington, D.C. Since many psychiatrists may not be familiar with the exact position of the American Psychological Association vis-à-vis psychiatry and psychotherapy, the reader's attention is called to this document. It is too complex to be summarized. I have selected the following three passages from it to give the reader a firsthand account of the psychologists' position in this controversy.

"The APA clearly states its intent to attempt to defeat amendments to basic science or medical practice acts that would restrict the role and practice of psychology. We have shown that such veto power can be mobilized; and, while it may not be a constructive step in interprofessional relations, it may be a necessary defensive maneuver.

"The APA goes on record as being ready where appropriate to join in the legal defense, and costs thereof, of any member engaged in professional practice who is charged with the practice of medicine in terms of psychotherapy. It is to be understood, in this connection, that one criterion of appropriateness is professional practice that is in conformity with the letter and spirit of our ethical code.

"The American Psychiatric Association, during 1957-58, rescinded its agreement regarding the acceptance of certification as the desirable and appropriate form of state legislation for psychology. In so doing, it has reverted to and reasserted an earlier official position that is ambiguous with respect to legislation for professions which it regards as ancillary to medicine, but which is com-

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clusions (only parts of which are cited here) were as follows:

> It follows that every area which does not at present have laws for certification or licensure of psychologists may anticipate a vigorous program by the American Psychological Association at the next session of their state legislature, and that an attack on existing medical practice acts is contemplated. In view of this, the Committee on Relations with Psychology of the American Psychiatric Association *RECOMMENDS THE FOLLOWING:*
>
> 1. Reaffirmation of the statement, "Psychotherapy is a form of medical treatment and does not form the basis of a separate profession." The psychological and physical components of an illness cannot be separated in diagnosis and treatment.
>
> 2. It is imperative that all psychologists dealing with persons suffering from mental and nervous diseases and disorders should do so only under supervision by psychiatrists and in a medical setting offering adequate safeguards to the patient."[10]

It is important to note that in the foregoing two statements the words "is," "does," and "cannot" are used, whereas, judging from the total message of the text, "ought" and "ought not" must have been intended. The analogy with prohibiting the use of alcohol could be pressed further in this connection. Thus, the toxicity of alcohol could easily be exaggerated, by stating that "alcohol *is* a poison," to justify the prohibition. This makes for a circular proof of this kind: 1. Alcohol is toxic; *therefore* its use *should* be prohibited. 2. Alcohol is prohibited; *hence* it *must be* a poison; otherwise the use would not be prohibited.

The Dissenting Opinion

I shall first state my opinion and then cite some arguments to support it.

1. The subject matter and methods of medicine and psychotherapy differ in fundamental ways. The former deals with the human body as a physicochemical machine; the latter, with man as a social being. The methods of the former are physical and chemical; those of the latter, "psychological," or, more precisely, communicational (i.e., talking and listening).[16,18,20]

2. The notion of mental illness is a myth. Psychotherapy cannot be said to be a method for "treating mental illness," since no such illnesses exist, except in a metaphorical sense. Mental, or so-called emotional, illness is a metaphorical term, used mistakenly as though it referred to deviations from widely agreed-upon criteria of "mental health." The criteria of mental health and illness are largely social and ethical; those of bodily health, anatomical, physiological, chemical, etc. The differences between the notions of bodily and mental health (and illness) thus appear to be much greater than the similarities.[11-14]

3. It is urged that proper differentiation be made between *institutional* and *instrumental* definitions of what constitutes medicine, psychotherapy, etc.[13,16,18] Thus, while it may appear that prohibiting nonmedical psychotherapists from independent practice may be in the interests of the medical profession or of patients, we cannot be *sure* that this is so. Too, the potential advantages of the prohibition itself (e.g., safeguarding the interests of patients) must be weighed against its possible disadvantages for the rest of society (e.g., abridging present freedoms in regard to counseling, advising, advertising, etc.). Since any "cure" may be worse than the disease for which it is employed, it would seem wise to face the problem of "bootleg psychotherapy" *before* it becomes an actual fact rather than after it.

4. In conclusion, it is suggested that the insistence of the medical and psychiatric profession on keeping psychotherapy within its domain and preventing others from participating in work of this kind may be a nearsighted attitude. While its immediate potential rewards for both physicians and patients may be real and tangible, there is reason to believe that the long-range effects of this social practice may be inimical to the best interests of both groups, not to mention society as a whole. If the

pletely unambiguous on the point that all psychotherapy is solely within the domain of the medically trained person."

modern psychotherapist is heir to the mantle of both physician and minister—as many believe him to be—then it would follow that his robe can be neither purely medical nor purely priestly, but should consist of some novel combination of the two, with materials from new sources (e.g., anthropology, philosophy, psychology, and sociology) added to it.

Comment

I have discussed various aspects of this vexing problem in several recent publications.[13-15] Accordingly, I shall only briefly indicate the main lines of my argument. For a more detailed discussion, the interested reader is referred to the articles listed in the bibliography.

I shall say nothing further concerning the subjects and methods of medicine and psychotherapy, respectively; nor shall I dwell on the problem presented by the notion of "mental illness." These have been adequately treated elsewhere. This brings us to a consideration of the differences between making an institutional versus an instrumental definition of a subject or process.

Institutional Versus Instrumental Definitions.—The notions of institutional and instrumental processes are fundamental sociologic and anthropologic conceptions. For our purpose it should suffice to indicate, by means of examples, what sort of concepts they are and what use we can make of them.

Consider a wafer. In a certain kind of setting (i.e., religious ceremony), used by a special sort of person (i.e., a priest), a wafer may be said to be a *sacrament.* In another setting (i.e., medical school), used by another person (i.e., a biochemist), it may be said to be a *carbohydrate.* Which one is it, *really?* This proves to be a meaningless question, for it can readily be shown that a wafer can be either, both, or neither, depending on *who* conceives of it and *in what setting.*

The crux of the matter is this. The notion of sacrament is an institutional concept. A wafer is a sacrament not because it is composed of carbohydrates but because of *how* it is used, *by whom,* and for what *purpose.* A wafer is said to be a sacrament if it is used as a symbol of—or proxy for—something else. If need be, some other object could be substituted for it, to serve the same purpose. Thus, almost any kind of food or drink may be defined as "sacred," and any spatial locus, as a "church," or "place of worship."

The notion of carbohydrate is an instrumental concept. A wafer is said to be composed of carbohydrates not because it is analyzed by a biochemist; or because the analysis is carried out in a laboratory, but because of the specific elements which it contains. This rests fundamentally on the examination of *facts* or of *evidence* and on the drawing of *conclusions.* When we say that a wafer is composed of *carbohydrates,* this word is not a symbol for "wafer," but, on the contrary, is a symbol for certain compounds containing the elements carbon, hydrogen, and oxygen. The processes necessary to determine whether something is or is not composed of carbohydrates are chemical. And when a chemical laboratory burns down, no other place except another chemical laboratory can substitute for it.

Another illustrative example will be offered, demonstrating the use of these concepts for classifying human groups. A kinship system—for instance, a family—is a typical example of a group based on institutional principles and values. On the other hand, task-oriented groups, such as a baseball or football team, are examples of groups based on instrumental principles and values.§

§ The contrast between institutional and instrumental groups is usually not as sharp as these brief comments might suggest. In general, this dichotomous classification is simply a means for codifying the well-known differences between tradition and precedence, on the one hand, and functional usefulness, on the other. Most human groups—medicine and psychiatry among them—contain both elements. The point of this discussion is simply

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Let us apply this distinction between institutional and instrumental principles and groups to medicine and psychotherapy. When a man suffering from headaches buys aspirin in a drugstore and ingests it, that is depreciatingly referred to as "self-medication." But when a physician prescribes aspirin, then it is called "medical treatment." Instrumentally, the two situations are alike; in both a chemical agent is ingested for the relief of symptoms. Institutionally, they differ; in the one, a drug is self-administered by a medically untrained person; in the other, a drug is given to a patient by a socially properly authorized person. Thus, just as only medication prescribed by a physician is considered to be "medical treatment," so, too, only psychotherapy conducted by a physician is recognized as "medical treatment." The statement, "Psychotherapy is a form of medical treatment," illustrates a definition based on institutional criteria. Instrumentally, however, psychotherapy continues to exist and to flourish as a nonmedical activity. Thus, nonmedical psychotherapy may be denied recognition by medicine or psychiatry, but it cannot be legislated out of existence. If this is granted, it may be very difficult to decide whether recognition of the facts or their official nonrecognition to safeguard certain principles is the wisest course to follow.

Value of Recognizing the Instrumental Criteria of Psychotherapy.—The official medical and psychiatric position thus far has emphasized the advantages of nonrecognition and ignored its disadvantages. In view of this, it seems worth while to counterbalance this view by stressing the value of recognizing psychotherapy for what it is instrumentally.

Sooner or later we shall have to face the fact that nonmedical psychotherapists have contributed most impressively to the theory and practice of this discipline. What follows should be interpreted neither as praise and agreement nor as condemnation and disagreement, with the particular ideas, teachings, and practices of any of the persons named. It is intended, however, as a forcible reminder of the "facts of life" in regard to psychotherapy.

Today, among the best-known psychotherapists in the United States, three are not physicians. They are Erich Fromm, Theodor Reik, and Carl Rogers. In England, there are Anna Freud, Melanie Klein, and a host of others, distinguished for their work, none of whom is medically trained. Many others could be mentioned—for example, August Aichhorn, Otto Rank, Hans Sachs, Robert Waelder, Ella Freeman Sharpe, Ernst Kris, Rollo May—who have achieved prominence not only as theoreticians but also as *independent practitioners* of psychotherapy. This being the case, there must be something amiss, surely, with an *ethical code* that allegedly proclaims that this ought not to occur, and yet where its occurrence is commonplace. Aside from this, from a scientific viewpoint, the question is: If psychotherapy is a medical specialty, how could so many medically untrained people contribute to its theory and practice? The example of biochemists, virologists, or geneticists comes to mind here, all of whom, while medically untrained, may be able to contribute to medical progress in a variety of ways. None of them, however, *practice* medicine. The crux of the difference, then, among the various scientists working in the so-called basic medical sciences, on the one hand, and psychotherapists, on the other, lies in the social exercise or application of their work.

The way out of this apparent dilemma lies through reexamining the precise nature of psychotherapy. If we do this simply and directly—and without befuddling the issue with such obscurities as "mental illness" or "psychological diagnosis and treatment"—then we arrive at approximately this an-

that scientific groups, insofar as they are "scientific," are supposed to be based preponderantly on instrumental principles. It is when they cease to be so constituted, and when institutional factors and values begin to predominate, that people begin to speak of "guilds" and "unions," using these words in a pejorative sense.[15,17]

swer. Psychotherapy consists of one person *influencing* another (or perhaps several others) by means of processes which may include one or several of the following means: talking, informing, advising, recommending, interpreting, explaining, promoting, exhorting, forbidding, threatening, and coercing. No doubt, this list could be refined or improved. My point is simply that any *instrumental definition* of psychotherapy is bound to include so many diverse processes that not only will it be impossible to restrict psychotherapy to medicine but it will be equally impossible to limit it to psychology. It is already widely recognized that many clergymen practice "psychotherapy." And what do the ladies who write the lovelorn columns of our daily newspapers do, if not "psychotherapy"? And so on down the line—parents, teachers, social workers, lawyers, student-advisers, advertisers, politicians, and others, all perform acts instrumentally indistinguishable from "psychotherapy." It would seem, then, that, rather than restrict this activity to physicians, the best course might lie in ridding our scientific vocabulary of this useless and misleading term. Once that is accomplished, we might be ready to consider the next problem, namely: Who should be officially designated by authoritative bodies as being specially trained to help people with their problems in living and under what conditions should he do this? At present it is difficult to consider this question rationally, because this job has, a priori, been assigned to physicians, who, however, are largely uninterested in, and untrained for, this role.‖

Supervision of Nonmedical Psychotherapists.—The last facet of this problem on which I wish to comment pertains to the supervision of the nonmedical psychotherapist. The American Psychiatric Association's recommendation expressly emphasized that psychologists "dealing with persons suffering from mental and nervous diseases and disorders should do so only under supervision by psychiatrists and in a medical setting offering adequate safeguards to the patient." Once again, we must hurdle the semantic barrier posed by the term "mental and nervous diseases." Does a person torn by a painful conflict over whether he should stay married or divorce his wife suffer from a "mental disease"? Or a student who does poorly in his work because he does not want his father to take pleasure in his achievements? Or a young woman who does not marry because she is afraid to be tied down to the job of caring for children? Let us proceed with the assumption that most persons who seek—and receive—psychotherapy nowadays do so for *problems in living.* If people wish to call these "mental illnesses," there could hardly be any objections. If, however, the very term "mental illness" is used to invoke the need for the psychotherapist's medical supervision, then objections are, in my opinion, well justified.

I submit, therefore, that to require that persons properly trained to do psychotherapy be supervised in their work by physicians or psychiatrists cannot be justified on technical grounds. The only rationale

‖ My own position in regard to this problem may be summed up as follows. I should try to seek out and support that course of action which seems to be most consistent with the values of (scientific) humanism. This would involve (*a*) an instrumental definition of psychotherapy, and (*b*) the licensing of persons to be officially designated as experts on the basis of technical training and competence. This would enable persons seeking psychotherapy to choose between those officially designated to do this kind of work and others not so qualified. Those not officially designated as "psychotherapists" would nonetheless be permitted by law to engage in this activity, although they would be barred from representing themselves as "officially qualified." The point here is that legal safeguards would be erected against falsely assuming an officially designated "role identity" (e.g., physician, attorney), rather than against engaging in a given act. It is worth noting in this connection that the commoner certain acts are (e.g., eating, drinking liquor, sexual relations, advising people in quandary what to do), the more hazardous it becomes for society to attempt to control too rigidly how they are to be practiced.

for this requirement lies in promoting the institutional superiority of medical psychotherapy over its nonmedical competitors. Competition is often praised or condemned, depending on whether one shares or rejects the ends which it seeks to realize. There is another way, however, in which competitiveness may be judged, namely, on the basis of the "rules of the game," by means of which superiority is sought. It is in this context that the notions of fair and unfair competition arise and obtain relevance. To require scientific and technical proficiency of persons rendering needed services—whether physicians, psychotherapists, or television repairmen—seems to be a good idea. Agreement on this value would lead to judging competition through superior proficiency as "fair." Contrariwise, attempts to improve one's position by means of status independent of technical competence would be judged as "unfair" competition. It behooves us to police ourselves, so to speak, both individually and collectively, in order to promote "fair play" on the part of as many players as possible. If we fail to do this, the "game" itself might easily become not worth playing.

It may be argued, finally, that medical supervision of psychotherapy is necessary because patients undergoing such therapy might be physically ill as well. That is true, but so could anyone else. Need we assume that a person who seeks psychotherapy is *ipso facto* incapable of exercising the *same care* for his bodily health as he would were he not seeking such help? The American Psychiatric Association's recommendation clearly postulates such a state of affairs, for the psychotherapy patient's *responsibility for his own health-care* is peremptorily wrested from him and transferred to his therapist. In part, there may be some historical grounds for this. Consider, in this connection, the evolution of Freud's own work.[3] Thus, for a time—and for many even today—the rules of the "medical game" were so defined that it was the physician's duty to watch, uncover, and treat the patient's problems, whatever they might have been. But for contemporary America, this is simply no longer true! Many medical psychotherapists today refrain from any and all kinds of medical procedures.[2] These physicians never physically examine their patients, nor do they ever prescribe drugs. Hence, many medical psychotherapists, while legally licensed to practice medicine, do not, in fact, themselves perform the physicochemical procedures characteristic of this discipline.[18] When patients in psychotherapy appear to require so-called organic medical care, they are referred to other physicians. In the case of persons undergoing psychoanalysis, or many other types of intensive psychotherapy, it is usually assumed—and often explicitly formulated—that the patient is responsible for the care of his health in just the same way as though he were not undergoing psychotherapy. Indeed, patients who are intact enough to be able to afford intensive psychotherapy, and who can participate in it, would rarely expect anything else than this. After all, the patient remains responsible for the conduct of every other phase of his life: his finances, his political or professional activities, his marriage, etc. Why should he not be responsible also for going to a (nonpsychotherapist) physician when he falls ill or is otherwise concerned about his health?

In view of all this, medical psychotherapists would be in need of medical supervision just as much as psychologists *if* the proper detection (and perhaps treatment) of bodily illnesses were considered to be a part of *their task!* If, on the other hand, physicians can legitimately limit their work to psychotherapy, why could psychologists not do likewise? The argument that the "therapist" (doctor) is responsible for the patient's "total health"—and hence that the psychologist must be medically supervised—holds only so long as we assume that he deals with persons so disabled or deranged that they are unable to discharge this duty themselves. Since there are such persons in the world, special legal safeguards for them are indeed desirable. Persons disabled

in these ways—for example, by toxic psychoses—represent, however, a small minority compared with the multitudes seeking psychotherapy nowadays. The existence of such conditions, moreover, is a matter of common knowledge. Their proper recognition and handling require professional responsibility and integrity on the part of psychotherapists—whether medical or nonmedical—rather than the subordination of some psychotherapists to medical supervision. Since this supervision is often carried out by psychiatrists—who themselves, by and large, are no longer especially competent in making "organic" diagnoses—we are compelled to conclude that this measure serves exclusively institutional interests.¶

Summary and Conclusions

My purpose in this article was to present some arguments against the contemporary official stand of medicine, psychiatry, and psychoanalysis concerning the proper role of the independent nonmedical psychotherapist. I have argued that this stand is similar to that of other prohibitionist laws, such as those aimed against alcohol, Negro voting, or free access to birth-control information and devices. In all of these instances, it is decided, by means of social action, that some bit of complex behavior should be (*a*) prohibited from general use, and (*b*) allowed under certain specific conditions. I have not sought to argue indiscriminately against this time-honored social practice. Rather, I have tried to show that it is often a matter of controversy as to which person's or group's needs are protected, and the needs of which others violated, by such legislation.

The limiting of independent psychotherapy to physicians would constitute a piece of restrictive legislation. Various reasons may be used either to justify or to condemn this proposal. To argue for this restriction, however, on the ground that "psychotherapy is a form of medical treatment" seems to me to be scientifically indefensible. It is like arguing for prohibition on the ground that alcohol is a poison.#

The medical, and especially the psychiatric, profession may be expected to show, by both logic and precept, that it does not confuse facts of nature with human decisions. The effects of chemical drugs or human acts—whether of alcohol, radioactive fall-out, or nonmedical psychotherapists—are one thing; social actions which we may take to enforce, allow, or prohibit them are another. While there are complex connections between the two, the latter can never be said to follow automatically from the former. Alcohol may be toxic in large doses, especially when used over long periods. This may or may not be construed to justify its prohibition. Cigarette smoking has not yet been prohibited; in fact, until recently, cigarettes were widely advertised

¶ These considerations also underscore the usefulness of *retaining* psychotherapy within the broad discipline of medicine. There are good instrumental reasons for having medically trained and qualified psychotherapists (i.e., "psychiatrists"). This does not mean, however, that psychotherapy need to be *restricted* to physicians. The practice of psychotherapy, officially legitimized, rather, is pictured here as existing both within and outside medicine. Consider in this connection the activities of such specialists as biophysicists, geneticists, or microbiologists. Their work may be carried out either in a medical setting (e.g., in a medical school, hospital, or medical research institute) or in a nonmedical one (e.g., in graduate schools of universities or in research institutes not specifically devoted to medical interests). Or, turning back to our analogy with prohibition, we find that, although alcohol is now generally available, nevertheless, it remains a useful "drug," too, which may be prescribed by physicians for its central nervous system or circulatory effects.

These considerations would be irrelevant for those who would argue on explicitly moral grounds that prohibition is desirable. This position rests on certain ethical premises, foremost among which is the notion that pain and suffering ennoble man, whereas pleasure leads him to perdition. Clearly, only a willingness to examine and alter this ethical orientation (and all that it implies) can change it. Similarly, there may be persons who believe, on the basis of tradition, that all the "healing" arts should be in the hands of physicians (and perhaps of the clergy). This position is based on historical precedent, rather than on the needs of a currently pressing task.

in medical magazines. By the same token, *even if* nonmedical psychotherapists could be shown to be inferior to their medical colleagues—which is surely a doubtful generalization—this *alone* could hardly justify restraining them from exercising their skills as best they might. Since there are really no guarantees in medical training or licensure per se that a physician is a competent psychotherapist, the patient's interests in the long run might be better safeguarded by educating him to heed the ancient Roman warning, *Caveat emptor*, meaning, "Buyer, beware!"* Adapted to the psychotherapeutic situation, the warning would read: "Patient, beware!" ("Consider carefully who your psychotherapist *is*, what he himself *believes* in, what are the terms of the *contract* under which he proposes to help you, etc. In brief, ask yourself: 'What kind of help am I buying, and for how much?'") I believe that in the present state of psychotherapeutic development no institutional guarantee can possibly replace this need for caution on the prospective patient's part.

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*George Herbert said: "The buyer needs a hundred eyes, the seller not one." Could we, in all fairness, not adapt this and say: "Patients in search of therapists need a hundred eyes; therapists in search of patients, not one."

Psychotherapy and Medical Responsibility

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"But all things excellent are as difficult as they are rare."[1]

"If psychotherapy is to be considered as any procedure or process which changes behavior or influences an individual towards a more adequate or satisfactory adjustment to his environment or encourages peace of mind, then its roots reach back into prehistoric ages."[2] The term itself is of more recent origin, first used by Reil in 1803.[3] In its broader sense, as above, psychotherapy is not strictly a medical discipline and is obviously the common professional property also of the clergy, teachers, social workers, and psychologists (I believe I am naming them in order of their numerical prominence, not necessarily implying any scientific rank order), and there need be no ghost from the grave to tell us this. A question arises, however, if we use as a more cogent definition: "Psychotherapy is a planned technique of altering the maladaptive behavior of an individual (or group) toward more effective adaptation. The essential ingredient of psychotherapy is the utilization of the therapist's personality interacting with the patient's personality."[4] Here we must examine the terms "maladaptive behavior," "effective adaptation," "therapist," and "patient." If the notion of therapist and patient are left out, the statement could apply as well to Chinese Communist "brainwashing,"[5] where the maladaptive behavior is allegiance to Western democratic ideals and the effective adaptation adherence to Eastern Communist ideals. However, the therapist-patient combination implies that more than social values are involved; that one person is helping another person whose difficulty causes pain for himself, directly or indirectly through breaking some of his own group's social sanctions. Traditionally, when we speak of someone as a patient, we imply further that he is, or should be, the recipient of medical care. Again, we run against a series of objections. Aside from the legal questions involved for psychopaths, for example, there are the moral questions of sin, guilt, and redemption, which some of the clergy would feel to be in their province. Of late some psychologists[6] would consider most persons whom psychiatrists regard as "patients" as persons with problems of learning, such as, for example, those with examination fear, marital discord, or homosexuality. Some psychiatrists[7,8] have raised the question whether psychotherapy does not more properly belong to the social sciences than to the medical sciences, whether we should not use the reference "psychosocial" rather than "psychological medicine."

I do not propose that these problems can be simply, quickly, or adequately resolved by legislative "fiat," or that the final decision will be settled by medical opinion. In the long evolutionary process of a developing science, progress is not orderly, is frequently subject to adverse social pressure (for example, Galileo), and inter- and intrascientific group rivalry. Certainly, although medicine is an old establishment, and psychotherapy, in its broader sense, is of antiquity, still what modern consensus we can achieve will come only when as many sides of the problem are considered as are feasible with our present instruments of scrutiny. For better or for worse, the chief instrument involved in what concerns us most in this field is not yet of the order of the probability type of verification of the

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physical sciences (nor may it ever be), since our chief measure is man himself.

Szasz, in a series of articles,[8-13] has questioned the relationship of the medical profession to psychotherapy. He has compared the request for medical supervision of psychotherapy to the prohibition of alcohol through the Volstead Act and the South African Segregationist liquor laws, arguing that this in an institutional rather than an instrumental judgment; that psychotherapy is instrumental rather than institutional in concept. Not only does he point out the difficulty of enforcing any legislation against "bootleg" psychotherapy, but contends that the nonmedical psychotherapists have contributed most impressively to the theory and practice of psychotherapy, that many conditions for which people come for psychotherapy are not "mental illnesses," and, moreover, that mental illness as a concept is a "myth." He separates medicine as a profession dealing with the human body as a "physicochemical machine" from psychotherapy, which deals with "man as a social being," and points out how little use is made of their medical training by physicians who practice psychotherapy.

These arguments are very challenging and stimulating. What is the reasoning behind the official position of the American Medical Association, the American Psychiatric Association, and the American Psychoanalytic Association, which is epitomized in the statement "Psychotherapy is a form of medical treatment and does not form the basis for a separate profession"? [14] That this is a political statement, as well as a scientific opinion, has aroused much controversy, and I would agree with Szasz [13] that the statement should be phrased "ought" and "ought not" if this is an institutional judgment, rather than "is," and "does not," unless one can show that psychotherapy really *is a medical treatment*, as distinct from a nonmedical, psychological, communications device.

Facts, as distinct from opinions, are hard to come by, and some of the facts of medical practice are not necessarily what we should support as psychiatrists. I would agree that many physicians consciously deal with the human body as a physicochemical machine and either implicitly or explicitly deny that they use their personality in reaction to the patient's personality both for diagnosis and for treatment. This is a fact we can all observe both in private practice and in some of our most respected medical-school colleagues. But, like dental caries, it need not be granted that this condition is healthy. Nor is it scientifically wise to separate "psyche" and "soma" to the extent that Szasz implies.

Medical practice should be conceived in terms of a comprehensive approach to the patient. The terms "organic" and "functional" are considered outworn, and felt emotion and its physiologic components should be considered a unit.[15,16] Yet, although the physiology of affects is an important background, I do not wish to beg the question of medical responsibility merely by invoking it, since, to use an analogy, even if we could quantify those neurophysiological components of the motivation and intellection of two chess players, still, I feel that the psychological motivation would be a more cogent explanation of the transactional process of chess. What is involved for medicine is the problem of diagnosis and treatment of the total patient in his social milieu. We are only at the beginning of a combined psychosomatic approach to the study of schizophrenia, ulcerative colitis, and a host of other illnesses. To segment the patient again in the old body-mind dichotomy would set the clock back immeasurably.[17]

There is bound to be a marginal zone where problems do not seem to be primarily "medical," such as speech impediments, reading difficulties, and some of the minor (but to the people involved, major) neurotic difficulties in social and sexual adaptability; but, even here, who is to decide that such a problem is the minor one? From my own experience, I have seen satyriasis with priapism in cases of leukemia and brain tumor as presenting symptoms; also, I re-

member the patient whose bilateral acoustic neuroma was revealed by a suicide which exposed the brain (the patient had been removed from the hospital against advice for a holiday weekend); the speech impediment which was part of a metastatic brain-tumor picture; the problem of headache in brain tumor even in a patient seen too early by the neurologist or internist, but at least picked up by the psychoanalyst shortly afterward. Perhaps these are rare examples, but I am sure that all of us, in our clinical experience, have had such traumatic occurrences.

What about the psychotherapy of the patient with peptic ulcer? Even if we have forgotten much "organic" medicine, we should recognize that if the patient is very pale and dizzy one morning, he may be bleeding painlessly. He may not be feeling ill enough to think of consulting the internist until later in the day or night, when his illness becomes obvious to anyone but when the situation has become quite grave. What about the ability to distinguish true vertigo from hysterical "dizzy spells"? These things do stick in our memory and make us loath to turn over complete, independent responsibility to either the patient or the nonmedical psychotherapist.

True, some of the medical background has certain negative value for the psychotherapist, unless he becomes aware of his countertransference bias, induced by such phenomena as the fact that his first "patient" was a cadaver, as Lewin has pointed out,[18] or that the taking of a medical anamnesis can be used to clear the doctor of guilt when he asks such questions as "What did you eat?; what were you doing when you first felt the pain?" Szasz claims[13] that people disabled by toxic psychosis represent a small minority and in any event the existence of such a condition is a matter of common knowledge. Is it? To differentiate "organicity" in terms of the symptoms of disorientation, confusion, confabulation, etc., requires a knowledge of the problems of differential diagnosis which psychologists, social workers, and ministers seldom, if ever, are taught. If we as psychiatrists admittedly miss cases of dementia paralytica (general paresis) or brain tumor, why should we then feel free to allow those with less experience to carry out unsupervised treatment? The principle of *caveat emptor,* assuming that we insult the dignity of personal choice inherent in our democratic, humanistic culture if we propose certain safeguards against unwitting suicidal self-treatment (or nontreatment), does not seem to accord with certain other well-recognized and accepted legal measures, such as compulsory vaccination for smallpox, sanitary inspection of milk and water supplies, or, to shift fields, government regulation of monopolistic economic situations. True, the public eventually decides these issues in a democracy by vote, but we are entitled to the best, at the moment, expert knowledge of the field involved before decisions are made.

The ethics of certain physicians are not what we desire; but at least we, as physicians, have a deeply inculcated respect for the welfare of our patients, deriving not just from our childhood patterns but from long, rigorous clinical background, with the 24-hour responsibility we assumed as students, interns, and residents. Again, I would admit, with Brody,[19] that some of this background tends to create too much acceptance of the role of omnipotence conferred on us by patients, to trade on transference rather than attempt to understand it. But he also points out, and I have seen this, too, that psychologists, for example, tend to be "theory bound" and that they sometimes withdraw into the excessive passivity of the "scientific observer," and have not had the opportunity medical students have had to work through some of their feelings about aggressive, frightened, or seductive patients. Not that we, as physicians and psychiatrists, are free from taint in these areas! Good supervision of psychotherapy certainly exposes these problems of both the medical and the psychological educational programs, and psychotherapy or psychoanalysis of the

would-be psychotherapist sometimes helps, together with supervision, to correct them.[20]

That there is plenty of room in our clinics and private practice for more psychotherapists and plenty of public demand for more adequate care, and also a more plentiful supply of it, is not disputed. But quality must not be sacrificed, even though demands are pressing. *"Primum non nocere"* is still a good motto for a healing profession. Intuition and good intentions are inadequate by themselves. As physicians, we must, however, learn to cooperate with our nonmedical colleagues in allied fields for our mutual benefit. To my mind, the theoretical contributions of such lay psychotherapists as Anna Freud, Erich Fromm, Ernst Kris, Carl Rogers, and Milton Wexler, to name only a few, cannot be ignored, nor can their reputation for therapeutic skill. (I do not intend any slur by use of the word "reputation," since I feel we likewise know very little about our psychiatric colleagues' actual psychotherapeutic skill, except after long acquaintance; and definitions of what is "success" in psychotherapy are moot.) Yet it must be considered that these people have functioned in a group setting in a clinic or university, or some have had close and mutually respected collaboration with a medical colleague. Again, I am sure that complete exceptions to this statement could validly be made, and there is no doubt that there are some intuitively gifted persons (history is a reminder of Christ, Buddha, David) who needed no college or institute.

The solution of our interprofessional (and intraprofessional) problems should not be approached via the Gordian-knot example of the complete separation of psychotherapy from medicine, or from an irrational segregationist viewpoint. We need desperately to combine forces against a common enemy of human suffering. More meetings of groups involved in the controversy must be held to work out a training and collaborative research and supervision plan which would allow scientific freedom and, at the same time, have adequate social controls to protect the public. It is startling to realize that the groups which are supposed to be most expert in human relations, which are depended on by government, business, and even sometimes the clergy, to have some answers for the preservation of man's humanity, that these same groups are very poor in the management of their own professional intergroup relations. It is worth considering further such plans as the one advocated by Kubie,[21] in his doctorate in medical psychology. It at least recognizes that the problem requires a new approach, and whether such a combined psychological-medical training is currently feasible is not as important as even its heuristic value for further investigation and stimulation of action. "Be not the first by whom the new are tried/Nor yet the last to lay the old aside," [22] is an inadequate research doctrine; yet it has attraction in the area of social responsibility.

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Influence of Lysergic Acid Diethylamide (LSD-25) on Subjective Time

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Introduction

Various reports [1,14] have shown that the subjective sense of time can be considerably altered through changes in the external circumstances under which the judgment is made. Somewhat less is known of the influence of the subject's physical state on his ability to estimate time. This lack may be at least partially due to the fact that most conditions, such as fever or delirium, which have been described as bringing about such changes often simultaneously make it difficult or impossible to collect systematic data. When physical states are experimentally induced, however, the effects can be better controlled and tested. In this way, information has been obtained on the influence of nitrous oxide [13] and mescaline [6,8] on the sense of time.

The very striking effect of lysergic acid diethylamide (LSD-25) on the sense of time has been described in various ways by those who have observed the reaction: "Time sense . . . was characterized by the feeling of being accelerated or retarded" [3]; "Not only is there impaired ability to estimate the passage of time, but there is also a sense of timelessness." [9] Yet Boardman and associates,[2] who studied systematically the judgment of time under LSD-25, found only an increase in variability and no change in the direction of the estimate. It appeared reasonable that, with such striking changes described as common to the drug, more definite results could be expected.

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Method

The subjects were 24 men who had been screened by physical examination, psychiatric interview, and group psychological testing as being within normal limits both physically and mentally. They were divided arbitrarily into two experimental groups of equal size, one tested first under LSD-25 and then acting as its own normal control (Group D-C), and a second, for which this order was reversed (Group C-D). The interval between the two tests varied from three days to a week. The drug testing was initiated two to three hours after ingestion of either 1μg. or 2μg. of LSD-25 per kilogram of body weight. Three subjects in each group were given the larger drug dose. Placebos were not given for the control condition.*

Four time intervals were chosen for testing: 15, 60, 120, and 240 minutes. Each subject was asked to estimate these same intervals twice, once under the drug and once in his normal state. Since it has been reported [4,5] that practice produces a change in ability to estimate time, the four intervals were presented in a different sequence to each subject. Thus, no particular time interval was in a more "favorable" position than the others when the results were summated.

The judgments under both conditions were made by the "method of production"; that is, the subject was told the length of time he was to estimate and then given a signal to begin. He was instructed to notify the experimenter when the designated interval had elapsed.

No limits were placed on the subject's manner of making his estimate except that he was not allowed to use his watch or to see the experimental apparatus, an electric stop watch. The experiment was carried out in a small room near

* Facilities were not available to give the subjects the bed care and attention during the control session which would approximate the treatment given during the LSD-25 reaction. It was decided that a partial duplication of the experimental treatment, such as substituting tap water for LSD-25 administration, would be an inadequate attempt at proper control.

TABLE 1.—*Median Time Judgments Under Drug and Control Conditions*

	Interval to Be Produced, Min.			
	15	60	120	240
Control estimate	13.5	58.0	112.0	212.5
Drug estimate	11.0	44.5	74.5	140.5

that occupied by the other subjects being tested the same day. As the room was not soundproof, the sounds of voices and a radio in the next room could easily be heard.

The data, a total of eight judgments per subject, four under the drug and four under the control condition, were analyzed by means of the Mann-Whitney test and Wilcoxon's matched-pairs signed-ranks test.[10] These nonparametric tests were chosen upon rejection of the assumption of normality and homogeneity of variance.

Results

Since the Mann-Whitney test did not show any significant interaction between the effect of the drug and the order in which the conditions were presented,[11] the data for Group D-C and for Group C-D were combined to determine the effects of the drug. Table 1 presents the median estimates for the two conditions. Under both experimental conditions there was an overestimation of the actual speed of the physical clock. However, the subjects judged the requested intervals of time to have elapsed more rapidly while under the drug than while under the control condition. Another way of stating this difference is that the subject's internal clocks ran more quickly under LSD-25 than in their normal states, so that, paradoxically, external events appeared to pass more slowly.

TABLE 2.—*Effects of LSD-25 on the Estimation of Time Intervals*

Interval, Min.	N	Sum of Ranks	Probability *
15	20 †	37	0.01
60	24	58.5	0.01
120	24	24.5	0.001
240	24	40.5	0.01

* Two-tailed test.

† Following Siegel,[10] four cases were omitted because the estimates under the two conditions were equal.

The results of the Wilcoxon matched-pairs signed-ranks test for drug effect are shown in Table 2. At each interval the subjects' time judgments were significantly lower under the drug than under the control condition. At 15, 60, and 240 minutes the probability is less than 1 in 100 that the difference is due to chance; at 120 minutes the probability is less than 1 in 1,000.

Comment

The results of this experiment are in agreement with published descriptions of the LSD-25 phenomena in finding a disturbance of normal temporal judgment. While the previous descriptions emphasize change in conceptual time, these results point out not only a difference in function but also a rather consistent direction of the change. Time does not merely pass differently; it appears to pass more slowly than usual. It must be remembered, however, that the findings here are limited to a specific time period of two to three hours after the ingestion of LSD-25. It is conceivable that other intervals during the drug's relatively long span of effect may manifest a different or less consistent temporal change. For example, it is possible, that the drug initially slows a pertinent physiological process and then speeds it later in the drug sequence.

Such a change in the effect of the drug may explain the lack of consistent error in judgment reported by Boardman, since the data of that experiment were collected over a broad range of the activity of the drug, rather than at a specific time in the reaction. However, there are other hypotheses which may explain the discrepancy between the results reported here and those of Boardman and associates. For example, the latter group studied the ability to estimate a one-minute period, whereas the present study focused on longer periods. Very little is known about the mechanisms used to judge time, and it is quite possible that the judgment of very short periods involves different clues and abilities than those necessary for judging longer intervals.

Perhaps a more serious difference between the present study and that reported by Boardman is one of procedure. In the earlier study the subject was presented with a series of stimuli which differed, one from the next, by 0.1 minute. This series was presented in alternately ascending and descending order, and the task was to judge whether each stimulus was greater than or less than one minute. Such a presentation may well have introduced the extraneous factor of perceptual rigidity, i. e., difficulty in perceiving a change in a stimulus when it is altered very gradually. Perceptual rigidity has been found to relate to a concrete attitude on the part of the perceiver,[7] and it is known that LSD-25 does produce a greater concreteness in thinking.[12] An increase in perceptual rigidity might explain the increased variability with no constant change in direction which was found by Boardman.

Granted that the results reported here do reflect a change in temporal perception, it is difficult to ascertain a reason for that change. One explanation of temporal perception is based upon the assumption that there exists one or more physiological regulators, i. e., heart rate or rate of breathing. Thus, when the physical state is altered in a manner affecting the regulator, or internal clock, time would appear to pass at a different rate. Certainly, since LSD-25 does disturb the normal physical state, this interpretation cannot be rejected.

Another hypothesis often called upon to explain changes in the subjective passage of time involves the subject's evaluation of how the time is spent. To give a few greatly simplified examples: One may expect a dull or painful period to pass slowly; an interesting one, quickly; an active one, quickly. In that the time spent under the influence of LSD-25 is generally considered very different from usual, and is frequently reported as unpleasant, a judgment made under the drug that time appears to be passing slowly would also be consistent with this psychological interpretation of the phenomena.

While the preceding paragraphs suggest a number of relatively specific factors which may account for the observed disturbance in temporal judgment, the point should be made that temporal orientation may well be dependent upon a combination of factors rather than being a function of any single one. Even such an apparently simple judgment as the localization of an auditory stimulus has been found to depend not only upon auditory cues but also upon visual cues and expectations built upon past experience. It is a reasonable hypothesis that temporal judgments may be based on the utilization of an even more complex array of cues, and that either the failure to receive cues or the failure to integrate them properly may result in a disturbance of temporal orientation. Thus, the observed effects of LSD-25 on temporal judgment may be not only concurrent with but also directly dependent upon the extensive impairment of perceptual and conceptual functioning which the drug is known to produce.

Summary

Twenty-four subjects were used as their own controls to determine the effect of lysergic acid diethylamide (LSD-25) on the subjective passage of time. Under the drug the subjects overestimated the stimulus intervals to a significant degree, indicating that the drug has the effect of making time appear to pass more slowly than usual. The results of this study were unlike those reported in an earlier experiment, in which no consistent changes were found under the drug. Differences between experimental procedures were discussed to suggest reasons for the divergent results. The findings were examined for theoretical implications in light of other changes noted as common to the LSD-25 reaction.

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Recall and Recognition Vocabulary

Implications for Intellectual Deterioration

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The major problem in evaluating the extent of intellectual deterioration has been to determine an accurate *post hoc* measure of the premorbid intelligence. Once this estimate of premorbid intelligence has been established, the extent of intellectual deterioration can easily be ascertained. The difficulty, however, has been in finding a valid measure of intelligence which remains relatively unaffected by deteriorative processes and which would be an adequate base line to which present functioning could be compared.

Methodologically, the ideal approach to decide which measure would best serve as this estimate of the original intellectual status would be to study patients before and after the onset of a disorder. The factor (or factors) which remain relatively intact can subsequently be used as the criterion measure of the premorbid intellectual level. Since this ideal experimental design of pre- and post-testing has been difficult to achieve, alternative designs have been used. Generally, there has been agreement that the vocabulary scale best serves as the criterion measure of premorbid intelligence. There are several reasons for this agreement: First, vocabularly has been found to be the best single estimate of intelligence, and, second, it has been found to be relatively unaffected by disease processes and the normal decline of functioning associated with aging.

Wechsler,[12] Rapaport and associates,[8] and Shafer,[10] as well as others, have discussed the relative stability of the vocabulary scale. Shakow [11] and others, however, have indicated that the vocabulary is also somewhat susceptible to deterioration, though not as severely as other functions. Yacorzynski [13] and, later, Feifel [4] pointed out that the relative stability of the vocabulary has been an illusion created by the crude scoring techniques, which failed to evaluate the level of abstraction and complexity of the definition given by the patient. Studies by Chodorkoff and Mussen [3] and Harrington and Ehrmann [6] demonstrated that the definitions of deteriorated subjects were consistently less abstract. However, when given a multiple choice of several alternative definitions, deteriorated subjects were found to select definitions at the same level of abstraction as normal subjects. Summarizing these issues, it seems there is general agreement that the vocabulary is relatively resistant to deteriorative processes, though there is a decrease in the level of abstraction and complexity of the definition. It was also noted that when a recognition type of multiple-choice vocabulary test was used, the vocabulary had an even greater resistance to the disease process. Glik [5] explored this interesting difference and tentatively concluded that the recognition vocabulary gave a more accurate estimate of the premorbid intellectual level. But Glik stated that his con-

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Recall and Recognition Vocabulary

Implications for Intellectual Deterioration

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The major problem in evaluating the extent of intellectual deterioration has been to determine an accurate *post hoc* measure of the premorbid intelligence. Once this estimate of premorbid intelligence has been established, the extent of intellectual deterioration can easily be ascertained. The difficulty, however, has been in finding a valid measure of intelligence which remains relatively unaffected by deteriorative processes and which would be an adequate base line to which present functioning could be compared.

Methodologically, the ideal approach to decide which measure would best serve as this estimate of the original intellectual status would be to study patients before and after the onset of a disorder. The factor (or factors) which remain relatively intact can subsequently be used as the criterion measure of the premorbid intellectual level. Since this ideal experimental design of pre- and post-testing has been difficult to achieve, alternative designs have been used. Generally, there has been agreement that the vocabulary scale best serves as the criterion measure of premorbid intelligence. There are several reasons for this agreement: First, vocabularly has been found to be the best single estimate of intelligence, and, second, it has been found to be relatively unaffected by disease processes and the normal decline of functioning associated with aging.

Wechsler,[12] Rapaport and associates,[8] and Shafer,[10] as well as others, have discussed the relative stability of the vocabulary scale. Shakow [11] and others, however, have indicated that the vocabulary is also somewhat susceptible to deterioration, though not as severely as other functions. Yacorzynski [13] and, later, Feifel [4] pointed out that the relative stability of the vocabulary has been an illusion created by the crude scoring techniques, which failed to evaluate the level of abstraction and complexity of the definition given by the patient. Studies by Chodorkoff and Mussen [3] and Harrington and Ehrmann [6] demonstrated that the definitions of deteriorated subjects were consistently less abstract. However, when given a multiple choice of several alternative definitions, deteriorated subjects were found to select definitions at the same level of abstraction as normal subjects. Summarizing these issues, it seems there is general agreement that the vocabulary is relatively resistant to deteriorative processes, though there is a decrease in the level of abstraction and complexity of the definition. It was also noted that when a recognition type of multiple-choice vocabulary test was used, the vocabulary had an even greater resistance to the disease process. Glik [5] explored this interesting difference and tentatively concluded that the recognition vocabulary gave a more accurate estimate of the premorbid intellectual level. But Glik stated that his con-

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clusions were limited because of his small sample and other methodological considerations.

The present problem was to explore this concept further and to compare the recognition and recall vocabularies of schizophrenic and normal subjects. The primary hypothesis of this study was that the recognition vocabulary of schizophrenic groups would approximate more closely that of a normal group than would a recall type of vocabulary scale.* If there were significant differences between deteriorated and nondeteriorated groups in the recall vocabulary, we could assume that this scale had been affected by deteriorative processes. If, on the other hand, these groups were not significantly different on the recognition vocabulary, we could assume that this scale had been relatively resistant to deterioration.

Procedure

Eighty schizophrenic inpatients were selected by the ward psychiatrist because they had met the following criteria: They (1) were under 45 years of age, (2) had been in the hospital for more than one year, (3) had not received any form of shock therapy within one month prior to testing, and (4) were able to verbalize sufficiently and were cooperative enough to take both tests. These 80 patients were rated by the psychiatrist as either mildly or severely deteriorated.

Of the 80 patients, 11 accepted the option of not participating in the research. The remaining 69 were individually given the Wechsler-Bellevue Form I Vocabulary Scale (recall vocabulary), followed by the Ammons Full-Range Picture Vocabulary (recognition vocabulary) in one testing session. This was the same procedure used by Ammons [1] in standardizing the Picture Vocabulary scale.

The standardization data presented by Ammons [1] were based on 120 normal subjects, and it is these data which are presented as the normal group in the present study. The two groups of schizophrenic patients, mildly and severely deteriorated, were compared with this normal group on each of the vocabulary scales by analysis of variance.

To test further the hypothesis, while controlling for possible sampling variations, the two groups of schizophrenic patients were matched with the normal group on the basis of mean and standard deviation of the Ammons Picture Vocabulary (recognition vocabulary). Assuming that this equated the groups for intelligence, the recall vocabulary (Wechsler-Bellevue Vocabulary) of these three matched groups were then compared by analysis of variance.

*A recognition vocabulary test requires the subject to select the correct response from several alternatives. On a recall vocabularly scale the subject must define the given word.

TABLE 1.—*A Comparison of Normal and Deteriorated Schizophrenic Groups on the Ammons Full-Range Picture Vocabulary Test (Recognition Vocabulary)*

	N	Mean	S. D.	F (2,186)	P
Normals	120	69.00	9.40		
Mildly deteriorated	34	69.26	12.65		
Severely deteriorated	35	65.69	11.98		
				1.44	N. S.

Results

As seen in Table 1, the unmatched groups were not significantly different on the recognition vocabulary scale. However, as seen in Table 2, the recall vocabulary decreased significantly with the independent estimate of the degree of deterioration ($P<0.001$).

As seen in Table 3, the groups were then closely matched on the mean and standard deviation of the recognition vocabulary scale. Table 4 presents the comparison of the recall vocabulary of these three matched groups, and the results indicate that the recall vocabulary still decreased significantly with the estimates of increased deterioration ($P<0.001$). The difference between the matched mildly and severely deteriorated groups was compared by the *t*-test, and this difference was significant

TABLE 2.—*A Comparison of Normal and Deteriorated Schizophrenic Groups on the Wechsler-Bellevue Vocabulary Scale—Form I (Recall Vocabulary)*

	N	Mean	S. D.	F (2,186)	P
Normals	120	23.40	5.80		
Mildly deteriorated	34	21.74	8.23		
Severely deteriorated	35	17.29	7.40		
				11.44	<0.001

TABLE 3.—*Matching of Normal and Deteriorated Schizophrenic Groups on the Ammons Picture Vocabulary Scale*

	N	Mean	S. D.
Normal group	120	69.00	9.40
Mildly deteriorated	25	69.04	10.37
Severely deteriorated	30	68.97	8.90

at less than the 0.10 level in the predicted direction.

The correlation between the Ammons Vocabulary and the Wechsler-Bellevue Vocabulary for the entire schizophrenic group was 0.86, which is identical with that reported by Ammons for the 120 normal subjects ($r=0.85$).

Comment

In a study of intellectual deterioration, one of the crucial steps is the estimation of the original intellectual status that existed before the onset of the disorder. Most research has indicated that of all measures of intellectual functioning, the vocabulary is least susceptible to the effects of deteriorative processes. It is for this reason that the vocabulary has been generally accepted as the base line from which one measures the degree of intellectual impairment.

The findings of the present study indicate that the traditionally used Wechsler-Bellevue Vocabulary Scale is also affected by deteriorative processes. A group of schizophrenic subjects were significantly lower on this vocabulary scale than were a group of normals. This difference between schizophrenics and normals correspondingly increased with an independent estimate of the degree of intellectual deterioration. This difference between the deteriorated and the nondeteriorated groups remained highly significant even after the groups had been matched on another type of vocabulary scale. When the groups were matched on the recognition vocabulary, which was hypothesized to be more resistant to deterioration, the groups were, in effect, matched for possible sampling variations. Even in this more rigorous analysis, the Wechsler-Bellevue Vocabulary Scale significantly declined with intellectual deterioration. Thus, the results support the contention of Shakow and others that the vocabulary test (the recall type of vocabulary) is also vulnerable to deterioration.

TABLE 4.—*A Comparison of Scores of the Matched Normal and Deteriorated Schizophrenic Groups on the Wechsler-Bellevue Vocabulary—Form I*

	N	Mean	S. D.	F (2,152)	P
Normals	120	23.40	5.80		
Mildly deteriorated	25	20.44	6.25		
Severely deteriorated	30	19.03	6.34	7.67	<0.001

In comparison with these findings on the Wechsler-Bellevue Vocabulary Scale, these three groups of subjects—normals, mildly deteriorated, and severely deteriorated—did not differ significantly on the Ammons Picture Vocabulary Scale. Though there was some decline in the score of the severely deteriorated group, this difference was not statistically significant. This finding confirms the contention of Glik and others that a recognition vocabulary is relatively resistant to deterioration.

Though it is not possible to conclude directly that the recognition vocabulary is a more valid estimate of premorbid intellectual functioning, the results do support this contention. To prove this point conclusively, it would be necessary to compare the recognition vocabulary of deteriorated subjects with a measure of intelligence obtained before the onset of the disorder. However, the fact that the schizophrenic groups closely approximated the normal group on the Ammons Picture Vocabulary indicated that this scale is relatively resistant to deteriorative processes, and, therefore, is a more valid estimate of premorbid intelligence than a recall type of vocabulary scale. If the Weschler-Bellevue Vocabulary Scale is to be used as an estimate of premorbid intelligence, a more liberal scoring system might be used for this purpose. In attempting to establish the original intellectual level, a subject should receive full credit if he has convincingly demonstrated familiarity with the word, even though the

level of abstraction of the definition has diminished.

One of the reasons for this difference between the two vocabulary scales may be that the recall vocabulary places little structure on the nature of the subject's response. Therefore, a schizophrenic patient who typically has difficulty in controlling his associative and thought processes, would be more vulnerable to this lack of structure. In the recognition vocabulary scale, both aspects of the question, the stimulus as well as the response, are structured to some degree for the subject. It is this structuring which may aid the schizophrenic patient to control his thought processes and thus allow him to function more effectively on the recognition type of vocabulary scale.

Summary

This study explored the difference between the recognition and recall vocabularies of normal subjects and schizophrenic subjects with intellectual deterioration. The primary assumption was that the recognition vocabulary (Ammons Picture Vocabulary) of schizophrenics would approximate more closely that of a normal group than would a recall vocabulary scale (Wechsler-Bellevue Vocabulary Scale). A group of mildly deteriorated schizophrenics ($N=34$) and a group of severely deteriorated schizophrenics ($N=35$) were given the Wechsler-Bellevue Form I Vocabulary Scale and the Ammons Picture Vocabulary and their scores were compared with those for a group of normal subjects ($N=120$). Though there was no significant difference among the three groups on the Ammons Picture Vocabulary (recognition test), the Wechsler-Bellevue Vocabulary (recall test) decreased significantly with an independent estimate of intellectual deterioration ($P<0.001$). The three groups were then matched on the basis of the Ammons Picture Vocabulary and still there was a highly significant drop on the Wechsler-Bellevue Vocabulary associated with the increases in deterioration ($P<0.001$).

It is suggested that the recognition vocabulary test lends structure to both the stimulus and the response aspect of the question, and it is this structure which facilitates the performance of the schizophrenic subject. In these findings there is support for the contention that a recognition vocabulary test gives a more valid estimate of the premorbid intellectual level than does a recall type of vocabulary scale.

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Some Evidence of a Relationship Between Hodgkin's Disease and Intelligence

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It has been a recurring impression [1,2] that patients with a diagnosis of Hodgkin's disease seem to be more intelligent than average. However, no objective data on a representative group of patients have been reported in the literature. With this in mind, 408 partial or complete clinical records of Army personnel who had received the diagnosis of Hodgkin's disease during the period 1942 to 1945 were studied for evidence of the general intelligence level of this group. Prewar occupation, which was listed in 209 of these records, was selected as most pertinent. In addition, as many of the Army General Classification Test (AGCT) scores of this group as were still in existence (97) were obtained by the Department of Physical Standards, Walter Reed Army Institute of Research.

Findings

1. *Army General Classification Test.*—The mean AGCT score for 97 white enlisted men with proved cases of Hodgkin's disease was 110.1, as compared with the Army population mean of 100 (standard deviation, 20). This difference was highly significant, as indicated by a *t*-test value of 4.75 ($P<0.001$).

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These records had been collected for an independent histological study and were kindly made available to us by Dr. Robert Lukes, of the Armed Forces Institute of Pathology.

Psychologist, the Ayer Foundation, Inc., and the Institute of Applied Biology (Dr. LeShan); Psychiatrist (Dr. Marvin), and Psychologist (Mrs. Lyerly), Walter Reed Army Institute of Research.

2. *Occupational Status.*—The clinical records yielded prewar occupation titles for 209 white enlisted men with Hodgkin's disease. In handling these data, we used as a basis of comparison Stewart's study of the relationship between prewar occupation and AGCT score,[3] in which she lists median AGCT scores for 227 occupations in a random sample of 81,533 white enlisted men.

In Table II of her study, Stewart [3] divides this occupational group into those whose AGCT medians fall into each one-half-standard-deviation interval from the mean of all the occupational medians. Similarly, we assigned the subjects in our sample to corresponding occupational groups and counted the number falling in each interval.

The accompanying Table shows, for each of these occupational groupings, the number of cases in the Army sample, the number of cases expected in the Hodgkin's disease sample under the null hypothesis, and the number of cases actually observed in the Hodgkin's disease sample.

We then tested the hypothesis that the proportions of persons in our sample falling into the several categories, as explained above, were similar to the proportions in Stewart's sample of 81,533. A χ^2 of 66.7 with 9 degrees of freedom for the data indicated that the differences in proportions were highly significant ($P<0.001$).

As an independent test which would serve to show whether these differences in proportions were directly related to the AGCT score, we computed the Spearman rank correlation coefficient between intervals from lowest to highest (the interval -2.4 to -2 receiving a rank of 1, etc.) and the propor-

Occupations of 209 Cases of Hodgkin's Disease and 81,533 Army Personnel Classified According to AGCT Medians Lying in Each One-Half-Standard-Deviation Interval from the Mean of All the Medians

S. D. Intervals	Occupational Groups	Army Sample *	Hodgkin's Disease Sample	
			Observed	Expected
2.0-2.5	Accountant, etc.	8,497	4	1.3
1.5-1.9	Auditor, teacher, etc.	8,661	12	2.6
1.0-1.4	Draftsman, pharmacist, etc.	12,022	13	7.3
0.5-0.9	General clerk, store manager, etc.	9,625	32	21.6
0.0-0.4	Salesclerk, linesman, etc.	20,316	19	24.6
−0.4-0.0	Airplane mechanic, butcher, etc.	9,586	38	52.1
−0.9-−0.5	Carpenter, welder, etc.	8,440	16	24.7
−1.4-−1.0	Baker, packer, etc.	2,861	28	30.8
−1.9-−1.5	Laborer, etc.	1,034	13	22.2
−2.4-−2.0	Teamster, miner, etc.	511	34	21.8
Total		81,533	209	

$\chi^2 = 66.7$.

* Stewart[2]: Table II.

tion of the number of cases in our Hodgkin's-disease sample to the number of cases in the larger sample for each of these intervals. The correlation coefficient was 0.67 ($0.05 > P > 0.01$). In other words, the proportion tends to be greater in those occupational categories whose AGCT scores are highest.

3. *Other Relationships.*—Wishing to check the possibility of a relationship between intelligence (or, rather, such correlates or reflections as were available to us here) and severity of disease, we tried two additional tests.

First of all, these cases had been classified, for purposes of another study,[4] into five histological categories, of no specific interest here except as they are said to differ in severity. We found no significant difference among these groups with respect to their AGCT scores in our 97 cases ($F < 1$ for 5 degrees of freedom).

In addition, a product-moment correlation coefficient between AGCT score (mean = 109.7; standard deviation = 19.3) and duration, in months, of disease (mean = 60.2; standard deviation = 51.2) from approximate time of onset to death or last reported condition was computed for 72 of these cases and was found to be −0.18, which is not significant. We concluded that if a relationship between severity of disease and intelligence exists, it is not reflected in our data.

It should be noted that the median age of this group of cases of Hodgkin's disease was 25.2 years, which is not significantly different from the central tendency of the Army population; thus, age can be ruled out as a contributing factor in these results.

Comment

Evidence has been accumulating in recent years that certain correlations exist between factors generally regarded as psychological and the development of neoplastic disease.[5,6] Although at this time little is known as to the meaning of these relationships, Meerloo[7] and others[8,9] have made some interesting speculations. We, however, prefer to offer these findings on Hodgkin's disease for their value as objective data, and to withhold our own speculations as to possible factors which might account for them.

Summary and Conclusions

The mean Army General Classification Test (AGCT) score for 97 white enlisted men diagnosed as cases of Hodgkin's disease during World War II was found to be significantly higher than that for the Army population mean. Of 209 cases of Hodgkin's disease among white male enlisted men,

the greater proportion of prewar occupations was found to be in those occupational groups whose median AGCT scores were highest. The data yielded no significant relationship between AGCT scores and severity of disease, as indicated by duration or histological type. We offer no interpretations or attempts to account for these findings.

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Relevance of Facial Expression and Posture as Cues in Communication of Affect Between Monkeys

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Some form of nonverbal communication is involved to a variable degree in every human interaction. Even verbal communication involves more than just the words that are used; intonation, speed of speech, gestures, and a variety of other factors modify the meaning of the words.[20,21] Further, every "intuitive" percept and every "empathic" relationship are dependent upon some nonverbal communication of affects. Although the precise nature of this form of communication is still obscure, it undoubtedly plays a significant role in the social relations among men,[22,23] among animals,[12,21] and between man and animal.[6,14]

In order to delineate the factors involved in the communication of affects, a method was devised for the experimental study of such communication among monkeys.[15] Thus, a monkey was conditioned to press a bar (conditioned avoidance response) at the sight of another monkey (conditioned stimulus) which served as a signal of impending electric shock (unconditioned stimulus). After the conditioned avoidance response was established, it was permitted to undergo extinction. Subsequently, exposure of the test animal to the sight of another apprehensive or fearful monkey reactivated the extinguished avoidance response in the viewer. The avoidance response had originally been conditioned as a defense against pain and was, therefore, a fear-motivated response.[16] Consequently, reactivation of the avoidance response indicates that the behavior of a fearful monkey is not only effectively discriminated from nonfearful behaviors but also elicits fear in the viewer, which is then manifested in performance of the defensive response, i. e., the conditioned avoidance.

The stimulus in the above study was the sight of a live, active monkey. During the critical trials for affective communication the stimulus animal exhibited many kinds of cues to the test monkey, e. g., cues associated with facial expression, posture, movement, and vocalization. While it is likely that this complex set of visual, auditory, olfactory, and other cues *in toto* is most effective in communicating affects from one individual to another, it became pertinent to determine the particular expressive details which combine in the larger amalgam of emotional behavior. Thereby it might become possible to weigh appropriately the relative contributions to affective communication of the various expressive modalities and to identify with greater precision the cue complex associated with a particular affect.

The present experiment was designed as a first step in identification of cues associated with the communication of apprehension or fear among monkeys. In order to accomplish this end it was necessary to isolate a single parameter of emotional expression from the larger complex of behavior. The facial-postural patterns of affect were selected for this more detailed appraisal. The isolation of these cues was effected by the simple expedient of em-

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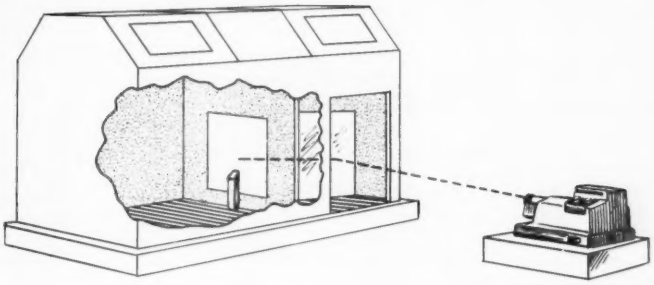

Fig. 1.—The apparatus with a cutaway view, showing the conditioning compartment and the projection screen. The projector, shown in its position outside the apparatus, was directed on to an angled mirror and thence on to the rear projection screen.

ploying as stimuli 35 mm. color transparencies of monkeys. These photographs were obtained from monkeys in "affectively neutral" situations, as well as from monkeys subjected to pain and/or fear situations. The use of these pictures made it possible to determine whether fear as expressed in the face and posture could serve as the stimuli for affective responses in the viewer.

Method

Subjects.—Eight young male rhesus monkeys were used as conditioning subjects in the experiment. These animals were experimentally naïve. They had been hand-tamed in the Wisconsin General Test Apparatus[12] for a period of six weeks prior to the beginning of the experiment. The hand-taming was conducted to accustom the animals to the experimenter and to train them to leave their living cages and enter the transport boxes.

Apparatus.—The conditioning apparatus (Fig. 1) consisted of a large rectangular box divided into two compartments by a specially constructed one-way vision screen, which made it possible to view the stimulus compartment from the conditioning compartment.[17] A response bar was located on the screen and was connected in such a way as to terminate both conditioned and unconditioned stimuli when pressed. Bar-pressing responses were automatically recorded on an electric counter. The grid floor of the conditioning compartment was electrifiable by means of a shocking device which delivered a pulsing shock that scanned the grid, thus eliminating the problem of shorting by fecal matter. The shock was automatically delivered to the grid by means of an Agastat relay three seconds after onset of the conditioning stimulus.

Behind the one-way vision screen in the stimulus compartment was mounted a rear projection screen. The color transparencies were projected on this screen by a Revere automatic slide projector located outside the apparatus and focused on an angled mirror. The picture on the rear projection screen was life-sized. This arrangement resulted in a remarkably realistic stimulus picture when viewed from the conditioning compartment.

Stimuli.—The conditioning stimuli (CS) consisted of a series of 35 mm. color transparencies which were prepared as follows: A monkey was placed in the stimulus compartment of the conditioning apparatus with the camera located in the conditioning compartment. After an adaptation period to permit the animal to become accustomed to the apparatus and the various photographic equipment, pictures were taken. First, a number of pictures were obtained with the animal in a "normal," unexcited state. Then various fear-eliciting stimuli, such as gloves, toy snakes, and shocks, were introduced so that pictures of fearful animals could be taken. Pictures were taken in the following categories.

1. Conditioned avoidance stimuli (familiar-calm)

This category consisted of seven pictures of Monkey 87 in calm, nonfearful poses. These slides were subsequently paired with shock in the avoidance conditioning phase.

2. Controls

Eight pictures of the empty stimulus compartment.

3. Stimulus-generalization stimuli

Five pictures of each of two animals (Monkey 85 and Monkey 90) calm and nonfearful. These slides were used in the stimulus-generalization and stimulus-discrimination phases.

4. Familiar-fearful stimuli

Five pictures of the conditioned stimulus monkey, (Monkey 87) in a fearful and excited pose.

5. Unfamiliar-fearful stimuli

Five pictures each of two animals (Monkeys 45 and 74) in fearful poses. These pictures were considered "unfamiliar" in that they had not previously been exposed to the test monkey and were included to determine whether communication of affect was common to any fearful monkey or was exclusive to the CS animal.

6. Unfamiliar-calm stimuli

Five pictures of each of two animals (Monkeys 45 and 74) in calm, unexcited poses. The unfamiliar-calm category was intended to provide data concerning effects on avoidance behavior of introduction of new or unfamiliar stimuli per se.

Procedure

Conditioning.—The test monkeys were first conditioned to perform an instrumental avoidance response upon exposure of the conditioned-stimulus pictures. The test animal was placed in the conditioning compartment for a one-minute adaptation period. At the end of this period the experimenter presented the first trial. Twenty trials were given each day during the conditioning phase: ten conditioning trials and ten control trials. The order of presentation of control and experimental trials was randomly determined for each conditioning day. A control trial consisted of exposure of a slide showing the empty stimulus compartment and was never followed by shock.

Each trial was begun by the experimenter's depressing a silent switch which activated the projection lamp and turned off the lights in the one-way vision insert. In this way, the conditioning subject could clearly see the picture projected on the rear projection screen. On control trials the experimenter manually terminated the stimulus after three seconds' exposure provided the animal had not already done so by pulling the response bar.

On conditioning trials the picture of the stimulus monkey was presented. After three seconds of exposure an electric shock was automatically delivered to the test animal until it made a bar-pressing response. A response during the initial three-second exposure period removed the stimulus picture, switched on the insert lights, and prevented the shock from being delivered to the grid.

The daily trials were determined as follows: Five conditioning stimuli and five control pictures were selected in a random fashion from the available slides in those two categories. These 10 slides were randomly interspersed in the automatic slide magazine. Position of the stimulus animal was systematically varied by the simple expedient of reversing the stimulus slides in the magazine on 50% of the trials. Thus, if the stimulus monkey was in the right corner of the stimulus compartment when the picture was taken, the procedure of reversing the slides insured that during conditioning the picture of that animal would appear half the time on the right side of the projection screen and half the time on the left. This procedure of reversing the slides on 50% of the trials was followed throughout the experiment.

Conditioning of the monkey to press the bar was facilitated during the initial trials by the use of the method of successive approximations.[17] This technique was continued only as long as necessary to teach the animal to make a bar-pressing response to terminate the shock. Conditioning trials were conducted until the subject met the criterion of 90% or more avoidance responses to pictures of the stimulus animal, with 10% or less bar-pressing responses on control trials for three consecutive days. If an animal had not met the discrimination criterion by 100 test periods, conditioning was terminated and extinction was begun.

Stimulus Generalization.—Animals which met the conditioning criterion were given a one-day test of stimulus generalization. Five pictures of the conditioned stimulus monkey were presented, followed by five pictures of another monkey in a calm pose. This sequence was then repeated once. Thus, the 20 trials consisted of 5 CS, 5 generalization stimuli, 5 CS, and 5 generalization stimuli. An extinction procedure was employed; i. e., no shock was administered if the test animal failed to pull the bar within three seconds. If no response occurred, the experimenter terminated the stimulus after three seconds of exposure.

Stimulus Discrimination.—Then a 30-day period of stimulus-discrimination training ensued. Blocks of 5 conditioned stimuli and 5 pictures of other animals were alternated for the daily 20 trials. Failure to press the bar on exposure of the CS pictures was followed by shock, but generalization pictures were terminated after three seconds and were not followed by shock. Pictures of two different animals (Monkeys 85 and 90) were used on alternate days. Sequence of slides within blocks was determined randomly. The stimulus-generalization and stimulus-discrimination periods were included in order to permit comparison with previously reported data.[18]

Partial Extinction.—As each animal completed the stimulus-discrimination phase, extinction of the conditioned avoidance response was begun. A series of five conditioned-stimulus pictures was shown twice on each extinction day. The pictures were not followed by shock and were terminated after three seconds in the event that the monkey had not responded by that time. Extinction was continued until the animal met the criterion of an average of 40% or less avoidance responses for three consecutive days.

Experiment 1: After meeting the extinction criterion, the test slides were exposed. There were four categories of stimulus pictures: (1) original CS pictures of Monkey 87 in a calm pose; (2) familiar-fearful pose: Monkey 87 in poses judged to be fearful; (3) unfamiliar-calm pose: pictures of Monkeys 74 and 45 in calm poses, and (4) unfamiliar-fearful poses: pictures of monkeys 74 and 45 in fearful poses. A sample from each of these categories is illustrated in Figure

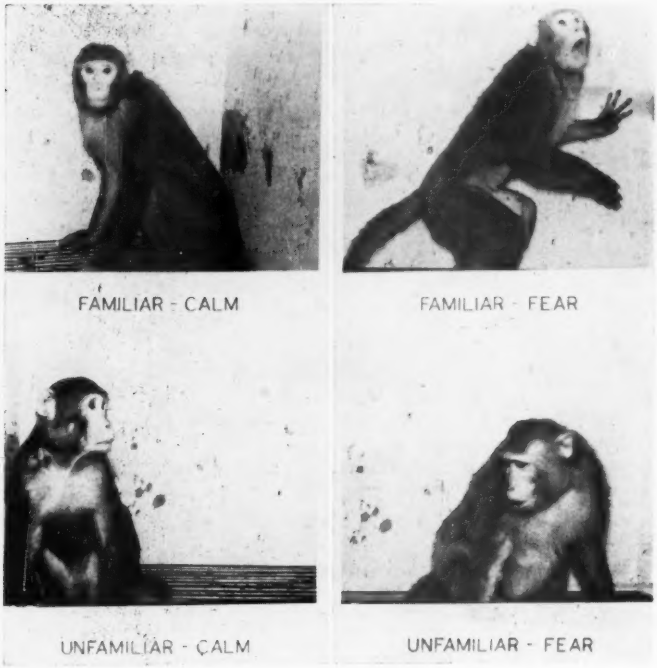

Fig. 2.—Representative pictures from each of the four stimulus categories used for communication-of-affect tests.

2. The four categories of stimuli were arranged in a 4×4 latin square. Two monkeys were tested in each of the four stimulus sequences. The sequence determined the order of presentation of the four stimulus categories. One set of stimulus pictures was presented on the first day; another category, determined by the latin square, on the second day, etc. The four stimulus conditions were thus presented in a rotation sequence lasting four days. Each daily test consisted of 5 slides from the appropriate category presented twice for the 10 daily trials. The 4-day rotation was presented at least three times (12 days) with the criterion that trials would continue until each category elicited no more than 10% of bar-pressing responses for three consecutive test periods. The exposure period was three seconds, and no shock was presented.

Spontaneous Recovery—Experiment 2: The animals were returned to the apparatus after a 30-day rest interval. Testing was resumed, using the same procedure as in Experiment 1. Animals were randomly reassigned to sequences in the latin square. As before, no shock was administered during this period.

Reconditioning.—When an animal had met the criterion of 10% or less of avoidance responses for three consecutive tests on each of the four categories, reconditioning was begun. The procedure and criteria were identical in every respect with those of original conditioning except that the maximum number of test periods was reduced to 40.

Experiment 3: After reconditioning, the test procedure was again introduced. Once again animals were randomly reassigned to sequence of presentation in the latin square. As before, an extinction procedure was followed, and the criteria of extinction were identical with those in the two previous phases where this procedure was followed.

Results

Conditioning.—Seven of the eight monkeys met the criteria of conditioning. The median number of test periods to condition was 72.5, with a range of 30 to 85. One animal (Monkey 110) failed to achieve the conditioning criterion within 100 test periods (1,000 presentations of CS and 1,000 controls). It was apparent that this animal was not adequately discriminating between CS and control slides, since he pulled the bar upon exposure of either stimulus. This bar-pulling response was very well established, and the level of avoidance responding to the

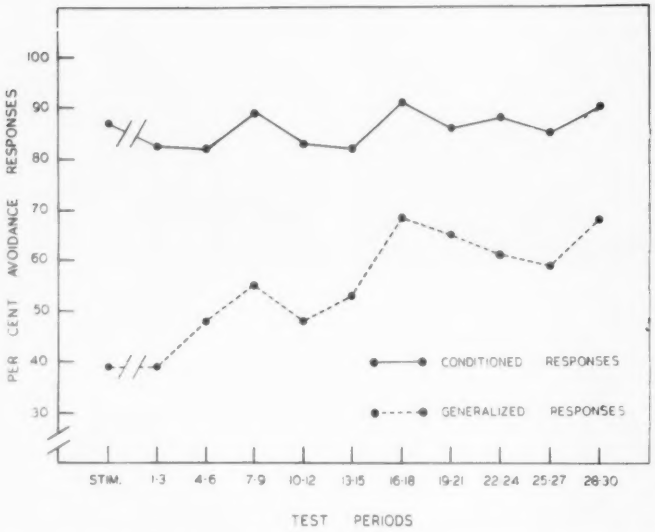

Fig. 3.—Performance of monkeys during stimulus-generalization and stimulus - discrimination training. The initial point, labeled *stim.*, refers to the one-day test for stimulus generalization.

CS was high (95% on the last 10 days of conditioning). Since this animal had already failed to establish a discrimination, it was started on partial extinction immediately without further testing on stimulus-generalization or stimulus-discrimination training.

Stimulus Generalization.—On the stimulus-generalization test the seven monkeys made a mean score of 87% avoidance response to the pictures used during conditioning (CS). The mean response to pictures of animals other than the conditioned stimulus was 39%. The difference in response level to CS and generalization pictures of 48% was found to be significant beyond the 0.01 level with the *t*-test.

Stimulus-Discrimination Training.—The stimulus-discrimination data are presented in Figure 3. This 30-day test period was designed to train the monkeys to discriminate between pictures of the CS animal and other animals. As can be seen in Figure 3, discrimination did not improve; in fact, the test animals generalized somewhat more at the end than at the beginning of the training period. A repeated measures analysis [7] indicated that the difference in response to CS and generalization stimuli was significant ($P<0.05$) during the first 15 days of training but was not significant thereafter. Alexander's trend test [2] showed no significant trend in the number of responses made to either CS or "neutral" animals.

Partial Extinction.—Immediately after completion of stimulus-discrimination training, nonreinforced presentations of the CS were begun. These extinction trials were continued until an average of 40% or fewer avoidance responses were made during three consecutive test periods. The median number of trials required to meet this criterion was 200 for the seven animals which had met the conditioning criterion. The range was from 50 to 570 trials. The animal (Monkey 110) which had not successfully completed conditioning, owing to failure to discriminate between the stimulus monkey and control slides (empty compartment), was extinguished to the 40% level in 170 trials.

Experiment 1: The group of animals was sharply dichotomized with regard to the response to the test pictures. Four of the animals extinguished immediately. A total of only 14 responses (median of 4) were made for the 480 trials presented to these animals. These same four animals had reached the criterion of partial extinction in

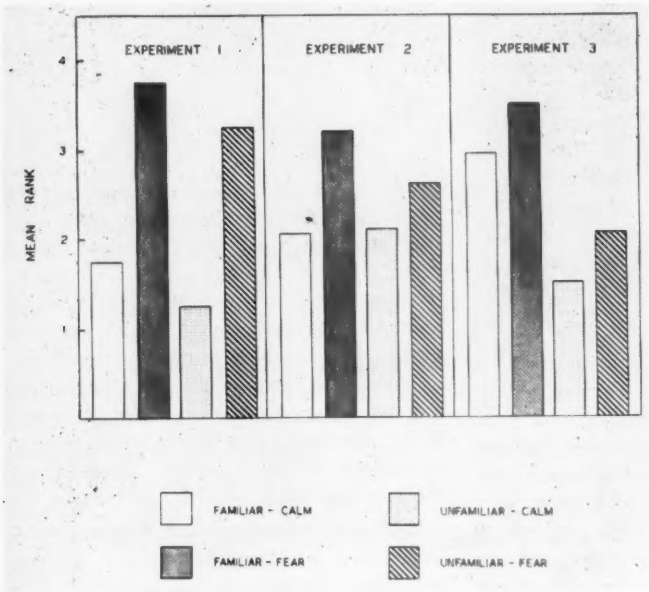

Fig. 4.—The avoidance behavior elicited during the communication-of-affect tests by the four categories of stimuli. The stimulus categories were ranked for each animal, the category eliciting fewest conditioned avoidance responses ranking 1 and the category eliciting the most responses ranking 4. These ranks were then averaged for each category and are shown as mean ranks.

the previous phase in a median number of only 80 trials (range of 50 to 200). The data from these monkeys were indeterminable with regard to the communication-of-affect hypothesis because too few responses were available for analysis.

The data for the four animals which did not extinguish immediately are shown in Figure 4. This group of monkeys had a median of 320 trials to the criterion of extinction for the communication-of-affect phase (range of 280 to 880 trials), with a median of 81 avoidance responses (range of 55 to 272). The Friedman test[8] indicated that the differences in response to the four categories of stimuli were significant beyond the 0.02 level. Pictures of the stimulus animal in fearful poses elicited the greatest number of responses, followed by unfamiliar-fearful, familiar-calm (CS), and, finally, unfamiliar—calm.

Recovery.—Experiment 2: Spontaneous recovery was begun after a 30-day rest period. As might be anticipated, the number of responses elicited by the stimulus categories during recovery was less than that during the initial experiment. The median number of trials to extinction was 140, with four animals extinguishing in the minimum of 120 trials. These data (Fig. 4) were not subjected to analysis because of the relative paucity of data.

Reconditioning.—The conditioned bar-pulling response was rapidly reestablished in all eight of the monkeys during reconditioning. The conditioned discrimination between control and conditioned stimuli, however, was not so easily relearned. Five animals failed to achieve the rather stringent criterion for the discrimination within the maximum of 40 test periods of reconditioning. However, a *t*-test revealed that the difference between the number of CR's and responses to control slides for the group of animals during reconditioning was significant ($P<0.01$), indicating that the monkeys were discriminating between control and CS pictures. A median number of 18 presentations of the noxious unconditioned stimulus (range 6 to 33) were received during this phase of the experiment.

Experiment 3: The third test for communication of affect was begun immediately after reconditioning. The results of this

test are shown in Figure 4. Since the response was at a high level, owing to reconditioning, a great many more responses were elicited during this phase than during the initial test, when partial extinction of the response had already occurred. The median number of test periods to extinction criterion for the eight animals was 40 (range 28 to 120). The median number of avoidance responses during this phase was 166 (range 68 to 816). The Friedman test indicated that the response to the various stimulus categories differed significantly ($P<0.01$).

Comment

The results of this experiment indicate that rhesus monkeys can discriminate between pictures of fearful and pictures of nonfearful monkeys. The finding that novel pictures per se (unfamiliar-calm) did not elicit significantly more avoidance responses than the CS (familiar-calm) indicates that this phenomenon was not an artifact which might be attributable to sensitization or some similar process. Further, the results of this investigation are in accord with a previous study,[15] which indicated that sight of a fearful monkey (or, in this case, a picture of a fearful monkey) evokes heightened avoidance behavior in the observing animal. In fact, more avoidance responses were elicited by pictures of animals in fearful poses than by the CS itself, which had actually been followed by painful shock during the conditioning phase. Since avoidance learning is presumed to be mediated by an acquired "fear" of the conditioned stimulus [16] one would conclude that more "fear" was aroused in the test animals by exposure of pictures of "fearful" animals than by the CS and/or by pictures of other animals in nonaffective poses.

The principle of stimulus generalization cannot account for the finding that more avoidance occurs to stimuli other than the CS. In fact, according to the stimulus-generalization hypothesis, one would have to predict that the CS would elicit the most responses, followed by the familiar-fearful pictures, with the unfamiliar categories both low and equal in capacity to elicit avoidance reactions. Stimulus generalization does appear to occur in a limited sense in that familiar-fearful pictures are more effective than unfamiliar-fearful in each test.

The results of these experiments appear to confirm the hypothesis that behavioral cues in isolation are not as effective in the communication of affect as the complex behavioral gestalt afforded by a live, affective animal. The measures of affective response to the slides employed in this study were not as strikingly clear as similar measures in the initial experiment with multiple cues.[15] It is anticipated that further reduction in stimuli will result in further decrements in the communication-of-affect phenomena.

Although these data show that emotional expression in face and posture is communicated among rhesus monkeys, the experimental evidence for a similar phenomenon in man is much less clear. The judgment of emotional expression in faces and postures is an area of investigation which has a long, and sometimes controversial, history. The many experiments dating from Darwin's [6] and Galton's [9] early works have not answered unequivocally the most fundamental question regarding facial expression of emotions: namely, can man accurately judge emotion in the facial expression of others? Undoubtedly much of the discrepancy in the data from these experiments can be attributed to differences in the type of stimuli employed (photographs of posed subjects, "candid" photographs, line drawings, etc.), as well as differences in the responses required from the judges (free responses *vs.* multiple-choice categories, etc.). Bruner and Tagiuri [5] have recently discussed these and other factors that have precluded unanimity in experiments which, superficially at least, are directed at the same question. Some broad generalization concerning the judgment of emotion from facial expression have been advanced.[5,27] While judges do not agree in placing a given facial expression in

closely associated categories, i. e., happiness and mirth, there is agreement when broader categories are used, and, for example, the judges designate an expression as happy or fearful. Woodworth reanalyzed several previous experiments on facial expression and constructed a scale of six nonoverlapping categories within which agreements among judges were high.[28] More recently, Schlosberg [24] confirmed and extended Woodworth's findings. Further, there is some evidence that naïve subjects can be trained to judge facial expression of emotions in agreement with the experimenter and with each other.[3,11] This, of course, does not necessarily imply that such training produces a higher percentage of "accurate" judgments in that consensus of judgments does not infer validity. Other experiments have indicated that judgment of facial expression is a developmental function [10] and has definite cultural determinants.[1,13]

While the study of facial expression of emotion has yielded relatively meager results, significant advances in the investigation of nonverbal communication in animals have been made by the ethologists. Their patient observation of animals in their natural habitat has paid handsome dividends in beautifully detailed descriptions of the social lives and struggles of a wide variety of fish, fowl, and mammals.[14,26] The intricate interplay of communicative acts which culminate in copulation, nesting, care of young, etc., are exemplified by Tinbergen's description of the courtship dance of the sticklefish.[26]

Perhaps one explanation for the striking and unexpected discrepancy between investigations of nonverbal communication in man and in animals is in terms of the kinds of measures employed in each approach. The ethologists have, by necessity, restricted their observations to motor behavior of communicating animals. In contrast, the investigations of facial expression of emotion in man have relied almost exclusively on verbal descriptions by the subjects. More recently, however, a formal system, kinesics, has been introduced which may permit more effective description and study of motor expression in man.[4] Clinical experience suggests that affect may be distorted or concealed in its translation to symbolic levels. Thus, it is possible that through use of autonomic or conditioned motor responses in an experimental paradigm similar to the one employed in the present experiments, a new approach could be made to the communication of affects in man. The question would be asked: "What does the judge himself feel upon exposure to a particular facial expression?" instead of the question: "What does the judge *think* the person in the photograph feels?"

Summary

Monkeys were conditioned to perform an instrumental bar-pressing response to a color slide of another monkey. During extinction, pictures of fearful and of calm monkeys were introduced. It was found that significantly more avoidance responses were made to fearful than to nonfearful pictures. Accordingly, it was concluded that the communication of affects in monkeys is demonstrable even when all behavioral expression other than posture and facial expressions is eliminated.

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The Reciprocal Effect

How Patient Disturbance Affects and Is Affected by Staff Attitudes

HAROLD A. RASHKIS, M.D., Ph.D., and ANTHONY F. C. WALLACE, Ph.D., Philadelphia

Since publication of "The Mental Hospital" by Stanton and Schwartz,[1] it has become a matter of wide interest in psychiatry that disagreements among staff members may tend to aggravate the condition of patients, specifically with regard to increasing the degree of overt disturbance. Stanton [2] has gone beyond this, recognizing that increased disturbance among patients may be, conversely, upsetting to the staff, tending to produce disagreement among staff members. Typical of this sort of disagreement is the situation in which a hospital patient becomes a serious problem to the nursing staff, who bring pressure on the medical staff to "do something." There are often differences of opinion as to whether the patient should be treated, for example, permissively or restrictively. Sometimes, when basic philosophies of treatment are challenged, serious rifts develop and become matters of grave administrative concern. This tension in high places is again communicated to the patient, who, in turn, becomes more disturbed.

The tendency for patient disturbance to upset the staff and for staff disagreement to upset the patient has been termed by Stanton the "reciprocal effect." There is little doubt that all psychiatrists who treat hospitalized patients have at one time or another experienced examples of this effect. In another paper we have considered in detail the question of the general demonstrability of one aspect of this effect—the impact of changes in staff consensus on disturbance in individual patients.[3] In this paper we shall evaluate another aspect of the effect—the relationship, not between changes, but between levels of consensus and disturbance.

Our first problem in attempting this evaluation was with semantics; i. e., what do we mean by "patient disturbance," and what do we mean by "staff disagreement?" We handled this in the following way.

EPPI Behavioral Disturbance Index

It is in the nursing staff (including attendants) that greatest sensitivity to patient disturbance lies. We therefore concluded that the nurses would be the appropriate "measuring device" for this variable. Our criterion for "one unit of disturbance" was any disturbing event involving a patient that was reported in the nurses' notes. It might be argued that we were thus measuring the nurses' disturbance rather than that of the patients. This is true, in part, with the consideration that nurses include in their notes not only what disturbs them but also what they think should be disturbing to them —or to the doctor—as evidence of disordered function in the patient.

We were reassured by the director of nursing at Eastern Pennsylvania Psychiatric Institute (EPPI), Miss Lillian Holmes, and by the director of nursing education, Miss Marian Chace, that nurses' notes are written solely for the use of the doctor, and that the notes would be written on his service in any way that he wanted. The nurses were accordingly lectured on how notes were to be written on the research unit, i. e., as simple behavioral descriptions of what happened, without interpretation or attribution of motives.

About a month after lecturing the nurses, we asked James L. Framo, Ph.D., and

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Arthur Adlerstein, Ph.D., if they would prepare from the nurses' notes a list of categories in which disturbances were reported. This list of 26 categories was prepared and revised, and reliability and validity studies were conducted. A satisfactory device was developed, called the EPPI Behavioral Disturbance Index, which will be published independently by Drs. Framo and Adlerstein.* On this Index the measure of patient disturbance is simply the number of incidents, according to certain criteria, cited per week, in these categories.

1. Aggressive disturbance (motor)
   (*a*) Against others
   (*b*) Against self
   (*c*) Against material objects
2. Aggressive disturbances (verbal)
   (*a*) Against others
   (*b*) Against self
   (*c*) Against material objects
3. Bizarre behavior
4. Complaining behavior
   (*a*) Whining, complaining
   (*b*) Overforbearing; self-denial
5. Compulsive disturbances
6. Delusional disturbances
7. Eating disturbances
   (*a*) Refusal to eat
   (*b*) Overeating
8. Feeling disturbances
   (*a*) Panic, anxiety attack, extreme fear
   (*b*) Flat, apathetic, dull
   (*c*) Inappropriate affect
   (*d*) "Nervous," anxious, upset
9. Hallucinatory disturbances
10. Manners disturbances
    (*a*) Unconcerned with social amenities
    (*b*) Overpolite, obsequious, ceremonious
11. Mood disturbances
    (*a*) Elated, exhilarated
    (*b*) Depressed
12. Motor disturbances
    (*a*) Overactive, pacing, running, agitated
    (*b*) Immobile, catatonic, posturing
13. Paranoid disturbances
14. Participation disturbances
    (*a*) Refuses to go to OT or group activities
    (*b*) Likes group activity too much; too active
15. Privilege disturbances
    (*a*) Irresponsible privilege behavior
    (*b*) Refuses privileges
16. Physical complaint disturbances
    (*a*) Complains of headaches, stomach aches, etc.
    (*b*) Indifferent to bodily injury, illness
17. Responsivity disturbances
    (*a*) Overdependent, clinging, helpless
    (*b*) Negativistic, resistant, stubborn, sullen
18. Sex disturbances (both homo- and heterosexual)
    (*a*) Masturbation, exhibitionism, seductiveness
    (*b*) Prudish, disapproving of sexual activities
19. Sleep disturbances
    (*a*) Oversleeping
    (*b*) Insomnia; has trouble sleeping
20. Sociability disturbances
    (*a*) Seclusive, withdrawn
    (*b*) Forces self on people; a "pest"
21. Thinking disturbances
    (*a*) Confused, disoriented
    (*b*) Perplexed, preoccupied
22. Tidiness disturbances
    (*a*) Sloppy in dress; untidy in toilet habits
    (*b*) Overly fastidious in dress or toilet
23. Verbal disturbances
    (*a*) Tangential, irrelevant speech
    (*b*) Rarely talks, mute
    (*c*) Loud, boisterous, shouting, overtalkative
24. Visitor disturbances
    (*a*) Overanxious about visitors
    (*b*) Negative about visitors
25. Ward routine disturbances
    (*a*) Does not cooperate in a ward routine
    (*b*) Overcooperates, seeks work unnecessarily
26. Miscellaneous (write in)

*The "Manual of Directions" may be obtained by writing to Dr. Framo

Patient Weekly Rating Form

In order to measure staff disagreement, we sought a device whereby we could determine "staff consensus." We decided that what we wanted to know about was the degree of like-mindedness among staff members so far as their attitudes toward individual patients were concerned. Participating in this investigation were an aggregate of some 40 patients and some 15 staff members. Since each staff member could have attitudes about each patient, there might be as many as 600 attitudes to be sampled on a number of separate occasions. Accordingly, we wished to keep down to a small number the questions that were asked of each staff member. A questionnaire was prepared by

one of us (A. F. C. W.), consisting of 10 statements selected out of a 71-item schedule devised by A. F. C. W. and Peter Lewinsohn, Ph.D., with which the rating staff member could agree or disagree. If staff members either all agreed or all disagreed with any statement pertaining to an individual patient, their consensus would be 1.00. Similarly, if all staff members showed the same pattern of agreements and disagreements for all 10 questions pertaining to a single patient, the consensus would, again, be 1.00. Consensus (C) was measured by the formula †:

$$C=1-\frac{4t(N-t)}{N^2}$$

where t is the number of staff members expressing the majority orthogonal attitude of agreement or of disagreement with statements on the questionnaire. Since for each question there could be only agreement or disagreement, consensus could be no worse than chance. The exact value of C depended on N, the number or raters.

The 10 statements on the Patient Weekly Rating Form were as follows:

1. This week he doesn't seem to understand what is said to him.
2. This patient truly is a very likable person.
3. As he is now, you have to treat him firmly and let him know who's boss.
4. Most of the other patients seem to like him pretty well.
5. Treatment doesn't seem to have been helping him this week.
6. I think this patient likes me.
7. This patient probably won't be able to leave the hospital for several months, at least.
8. He has been causing other members of the staff a lot of trouble this week.
9. He seems to get along well with the other patients.
10. This patient often has to be asked to do things.

One of these forms was given to each rater each week for each patient with whom he or she had contact, along with the following directions:

The purpose of this study is to increase our knowledge of patient behavior. We are interested in knowing how different patients appear to different staff members. Please indicate your agreement or disagreement about each of the 10 statements below by drawing a circle around the proper symbol: *A*, if you agree with the statement; *D*, if you disagree. Even if you are a little uncertain, choose one answer or the other. Answer spontaneously and freely; answer all the questions, and keep in mind that you are rating the patient about his behavior *this week*. There are no right or wrong answers; patients are all different, and different staff members are in a position to see different things.

† Developed by Mr. John Atkins, Department of Clinical Research, in consultation with Mr. James Casby, Department of Basic Research.

The Research Community

This study was conducted on a semidisturbed floor at the Eastern Pennsylvania Psychiatric Institute. This floor consisted of male and female nursing units, of about 20 beds each, which were administered by one of us (H. A. R.) along the now familiar lines of a "therapeutic community," very much in the manner described by Wilmer.[4] The unit was started for the purpose of the present study; hence its goal was research rather than therapy. Such a goal, of course, may be in itself rather therapeutic, as some of our earlier work has shown.[5]

The primary purpose of the administrator was to set up two well-functioning wards of semidisturbed patients. The principal form of active treatment was group psychotherapy for all patients who were not too disturbed to attend and, on the other hand, not well enough to have jobs outside the hospital. The manner in which group psychotherapy was conducted has been described elsewhere.[6] Occasionally we used tranquilizers, and rarely electroshock. Some patients received individual psychotherapy.

Staff meetings were held each morning, and special therapy conferences were held each week. Nurses, attendants, psychiatrists, psychologists, social workers, and occupational therapists attended these meetings.

From the point of view of administration there were two unique features to the management of this research unit. First, the administrator was never appraised of the data collected by means of the Disturbance Index and the Rating Form. Second, he made no active effort to reduce disagree-

ments which arose among staff members. There was, of course, a built-in mechanism to resolve disagreements, as the psychiatric residents were each responsible for their own patients. The administrator was present to support or advise the residents as required.

From an administrative and therapeutic point of view, the unit was fairly successful. The fact that it existed for a particular purpose, that the administrator was identified with clinical research, that psychologists were actively interested in the nurses' notes, that everyone had to fill in rating forms that were evidently important, that the patients were all getting group therapy, that there were adequate staff meetings, etc., etc., all combined to produce an excellent atmosphere that may well have been more therapeutic than if we had set out primarily to develop a "therapeutic community" with no specific research intent.

From a research point of view, however, we had, unfortunately, attenuated one of our variables: Our patients, by and large, were not very disturbed.

Procedure

After the unit had been running for a few months, we judged that the situation was fairly well stabilized, and began to collect data. On each Wednesday, Patient Weekly Rating Forms were given each staff member (psychiatrists, psychologists, nurses, attendants, occupational therapists) who had regular frequent contact with one or more patients; each staff member completed one form for each of his patients. We were careful that all staff members would work individually on this, and they did.

On the same day, using the nurses' notes, the Disturbance Index would be completed, listing the number of incidents for each patient for the preceding week.

As previously stated, none of this material was, in this form, available to the administrator; accordingly, he was not in a position to manipulate intentionally the outcome of the investigation.

Data were collected over a period of 12 weeks.

Results

We have already mentioned that our patients were not very disturbed. Just how disturbed they were may be seen in Figure 1, where the frequency distribution of the weekly disturbance scores is presented. Men with high disturbance scores were uncommon. Women, whose scores seemed to fall in a bimodal distribution, were more disturbed. A P of less than 0.001 on the Mann-Whitney U-test confirmed the sex difference.‡ This finding suggested that there was a better chance for Stanton's hypothesis to be confirmed with our female patients, whose disturbance scores covered a greater range, with greater potential for variation. These distributions, however, told

‡ Mrs. Martha Ann Teghtsoonian did much of the statistical work reported here.

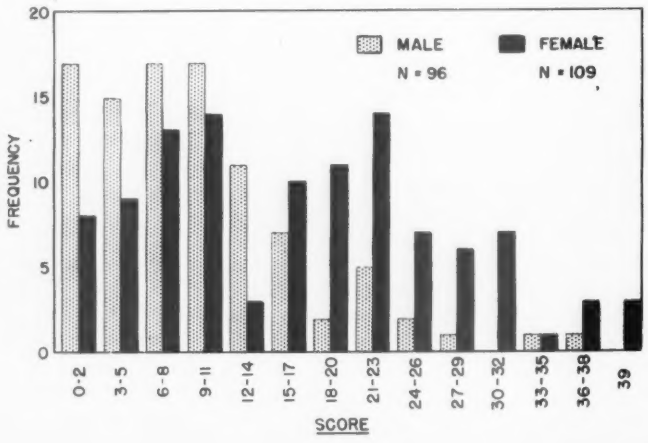

Fig. 1.—Distribution of disturbance scores for male and female patients.

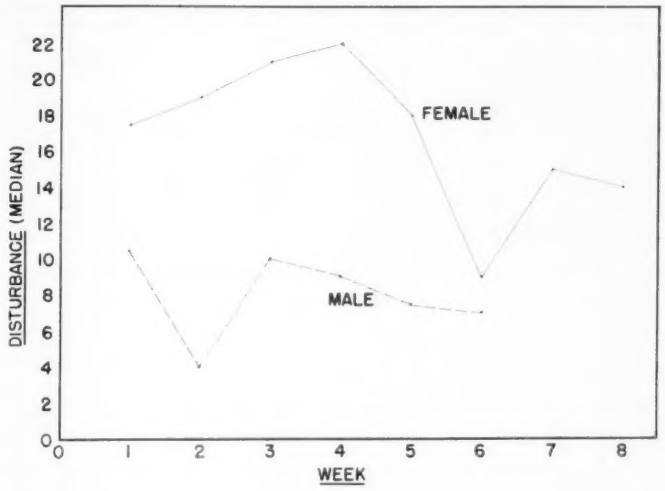

Fig. 2.—Weekly disturbance scores, male and female patients.

us nothing about the variability of scores on a given individual, or about the direction in which the scores were moving, i. e., whether patients were getting "better" or "worse" (less or more disturbed) with the passage of time.

Figure 2 shows the disturbance scores recorded on successive weeks. Male and female scores are shown separately, since they were found to represent statistically different distributions. The over-all impression is that of reduced disturbance in both groups, though the course is far from smooth.

Turning to the consensus scores in Figure 3, we see, by and large, the same trend, reflected in increasing consensus with the passage of time. Accordingly, we are able to conclude that both disturbance and consensus scores tended to move in what might be called a "favorable" direction with the

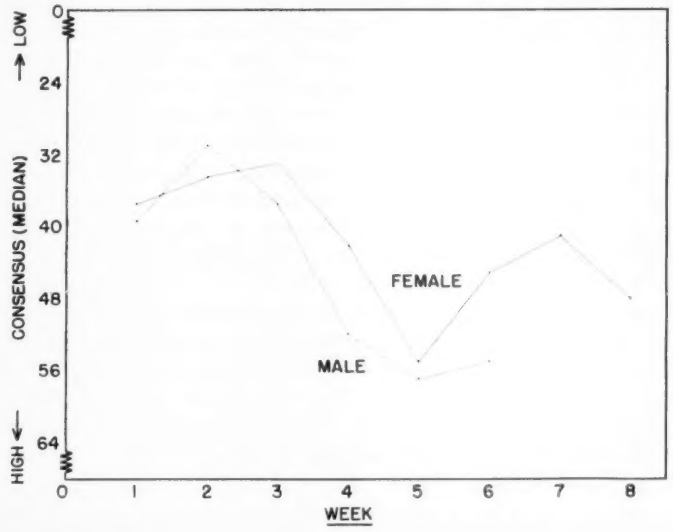

Fig. 3.—Weekly consensus scores, male and female patients.

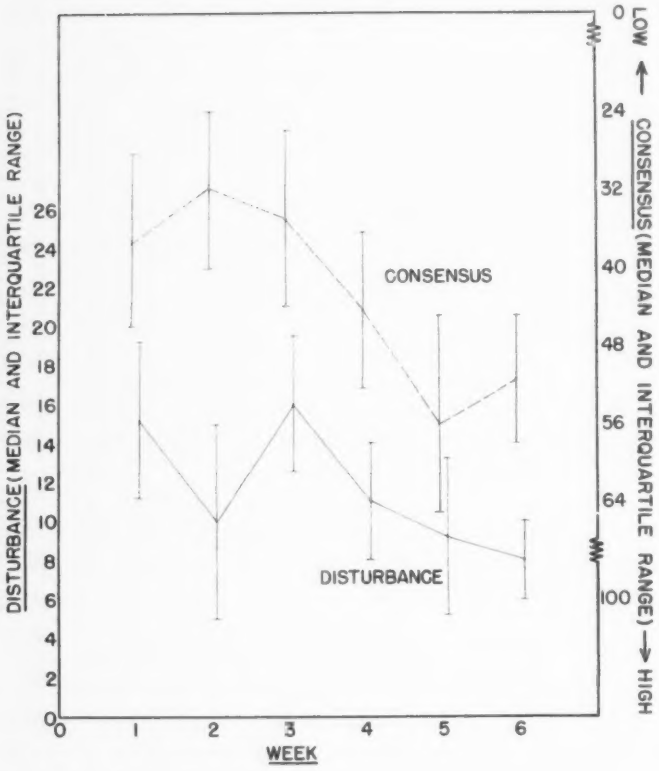

Fig. 4. — Changes in disturbance and consensus (male and female patients) as a function of time.

passage of time, under our particular conditions, as we see in Figure 4.

We have included data only for six weeks for male patients and for eight weeks for female patients. We have data that cover 12 weeks, but the data reported are the longest runs that we have on any group of patients (N=12 males and 12 females). Unfortunately for our project, our patients tended to get well enough for discharge in six to eight weeks, and it is not our philosophy to hold patients in the hospital purely for research purposes. This reflects the current trend for shorter periods of hospitalization; if consensus of staff attitudes is to influence the course of hospitalization, it has to act promptly. This reminds one of the plight of the resident, intern, or medical student, whose ward patients sometimes get discharged before he has a chance to know them.

It is now necessary to raise the crucial question as to whether increased consensus is temporally responsible for (i. e., precedes) reduced disturbance or vice versa. The evidence our data provide serve somewhat to support both these views.

To consider the original proposition of Stanton and Schwartz: Does increased disturbance follow staff disagreement—or, as we phrase it, decreased consensus? To answer this question, we sought to relate disturbance scores for week n with consensus scores for week $n-1$, i. e., for the preceding week. Lacking a method of instantaneous recording which would enable us to record consensus at time t and disturbance at $t+x$, this seemed the most reasonable approach. Probably it takes several days for staff attitudes to dialyze across to the patients. If these time relationships were ever determined, it would represent a consider-

TABLE 1.—*Consensus Predicting Disturbance: Male Patients*

		Consensus (Week $n-1$)		
		Below Mdn	Above Mdn	Total
Disturbance (Week n)	Above mdn	26	13	39
	Below mdn	12	26	38
Total		38	39	77

TABLE 2.—*Consensus Predicting Disturbance: Female Patients*

		Consensus (Week $n-1$)		
		Below Mdn	Above Mdn	Total
Disturbance (Week n)	Above mdn	25	18	43
	Below mdn	21	25	46
Total		46	43	89

able step toward putting social psychiatry on a scientific basis.

By taking the median consensus and disturbance scores for weeks $n-1$ and n, respectively, and splitting the points into four groups with direction appropriate to the hypothesis, we obtain Table 1 for the male patients and Table 2 for the female patients. For the male patients χ^2 is 8.12, with $P<0.01$. For the female patients χ^2 is 0.932, with $P>0.05$. Accordingly, there is a significant relationship, in these data, between disturbance and the preceding week's consensus for the male patients but not for the female patients, thus offering partial support for Stanton and Schwartz' observation.

We may now consider the alternate question: Does decreased consensus follow increased disturbance? Similarly treating the data, we obtain Table 3 for the male patients and Table 4 for the female patients. For the male patients χ^2 is 5.26, with $P<0.02$. For the female patients χ^2 is 3.19, with $P>0.05$. Accordingly, there is, once again, a significant relationship for the male patients but not for the female patients.

Accordingly, we see that there *is* an apparent reciprocal effect, that it is significant for the male patients but insignificant for the female patients.

Comment

Cautiously, we have stated that there is an *apparent* reciprocal effect. For the male patients, high or low consensus seems to precede low or high disturbance; low or high disturbance seems to precede high or low consensus. Either can precede the other; the chicken comes before the egg and the egg before the chicken. This effect is not significant for the female patients.

There are two separate ways in which to account for these findings. The first of these is, simply, that both male and female patients were improving, as a result of therapy, the males more markedly, while consensus was increasing, as a result of increasing familiarity with the patients, but that staff opinion had little or no reciprocal relationship with the improvement. It does seem to be true from our data that staff consensus increases with the passage of time and that disturbance falls as the patients approach the time of discharge. This hypothesis to account for the reciprocal effect—that even in the absence of any dynamic interaction between the variables, the data would ar-

TABLE 3.—*Disturbance-Predicting Consensus: Male Patients*

		Disturbance (Week $n-1$)		
		Below Mdn	Above Mdn	Total
Consensus (Week n)	Above mdn	25	16	41
	Below mdn	12	25	37
Total		37	41	78

TABLE 4.—*Disturbance-Predicting Consensus: Female Patients*

		Disturbance (Week $n-1$)		
		Below Mdn	Above Mdn	Total
Consensus (Week n)	Above mdn	27	17	44
	Below mdn	19	28	47
Total		46	45	91

range themselves so as to suggest a spurious correlation—is supported by the results of the study[3] cited earlier. This investigation showed that when change in consensus, positive or negative, is matched with change in disturbance, positive or negative, patient by patient, no significant relationship can be found. In other words, *change* in consensus does not seem in itself to elicit any predictable *change* in disturbance of either the highly disturbed or the minimally disturbed patient, either male or female.

But while this hypothesis, among others, would explain the presence of the apparent reciprocal effect among the males, it does not explain the *absence* of this effect among the females. To be sure, the female data show an empirical tendency to arrange themselves in the same manner as the male data: but the tendency is not statistically significant. Furthermore, the study reported in this paper dealt with a slightly different aspect of the reciprocal effect from that considered earlier: the relationship between *levels* of consensus and disturbance, the criterion of level being the group median.

Let us therefore approach the problem of the analysis of the reciprocal effect by another route. Suppose our patients had not been improving—would we have then seen a negative relationship? Looking back over our data, we note that four of our patients were slightly "worse," i. e., more disturbed, at the end of our investigation than at the beginning. Three of these were male and one was female, despite the fact that the male patients showed more over-all improvement. For the three males the consensus scores were seen to be rather closely correlated with disturbance scores; for the one female patient, consensus improved while she got "worse."

This finding further substantiates the opinion that the reciprocal effect holds for the male and not for the female patients. This leads to the second hypothetical explanation for our data, namely, that male patients interact more closely with the staff than do female patients, that staff and male patients perceive each other more clearly than do staff and female patients.

This last hypothesis resolves itself into two parts. First, does the staff view the female patients differently from the male patients? We have data which bear on this question.

Three staff members rated both male and female patients. We have also adequate data on eight staff members who rated only male patients and eight others who rated only female patients. Referring to the Patient Weekly Rating Form, we note that it is possible to adopt a "Pollyanna" attitude, wherein one agrees with Statements 2, 4, 6, and 9 and disagrees with Statements 1, 3, 5, 7, 8, and 10. On each statement for which this is not done, the patient may be said to have received a "black mark." We ranked staff members on their tendency to give "black marks" to male patients and their tendency to give them to female patients. We also ranked male patients and female patients on their tendencies to receive "black marks." As we were interested in the homogeneity of these matrices, we calculated the Kendall Coefficient of Concordance, and obtained W's ranging from 0.525 to 0.59. Each of these was significant with $P<0.001$. We learned that raters tend to be consistent in ranking different patients, and that patients tend to be consistently ranked by different raters, regardless of the patient or the rater. Rankwise, there was no evidence of "discrimination," in the opprobrious sense. We further compared the ratings by the three who rated both males and females against the eight who rated males and the eight who rated females, using the Kolmogorov-Smirnov Two-Sample Test. The three (two psychiatrists and a nursing supervisor) who rated both males and females were found to give the females significantly more black marks than they gave the males, and were found to give all patients more black marks than did each of the two sets of eight raters each. However, the eight who rated only female patients gave them more black marks than did the eight who

rated only male patients. *P*'s ranged from <0.001 to <0.05 and were all significant. We have sufficient data to conclude that some raters are "harder" than others; some patients receive consistently "harder" ratings than others; female patients are ranked more "harshly" than are male patients.

Since the female patients were more disturbed and were ranked more harshly than and just as consistently as were the male patients, it is surprising that the reciprocal effect was not confirmed with this group. This leads us to what we had expected to be our final consideration, which should, but does not, unravel the mystery. That question is, do female patients perceive the staff differently than do male patients?

On this point we can offer no direct evidence. We can suggest only that there may be a failure in communication between staff and female patients in our study. It is easy enough to say that when women become mental patients they "just let themselves go" and shut themselves off more completely from environmental influences than do men, but we are not now prepared to offer experimental evidence that this is the case, nor are we now prepared to explain why this might be so.

Another kind of explanation, for which we have no evidence, is that the staff, though rating female patients "harshly," are not "harsh enough," and tend to excuse periodic fluctuations in the behavior of female patients as part of "woman's capriciousness." The permissiveness may be further communicated to the patient in a failure to communicate staff disagreements, resulting from a "patient-sparing" attitude directed selectively toward female patients. It may well be that the female patient not only is "sicker" and less "reality-oriented" than is the male patient but is also deprived of the benefit of rectifying feed-back from a staff that is attitudinally "hard," but behaviorally "soft," toward them.

The preceding hypotheses are only two of many that might be offered.

Accordingly, we see that there may be not one but two different "reciprocal effects," one for the male patients and another for the female patients. The former of these is that described by Stanton. The latter we know only by our inability to demonstrate the "reciprocal effect" with the female patients, and can only speculate on its nature.

Summary

Male and female nursing units are established in an attempt to study the "reciprocal effect," i. e., the effect of staff disagreement on patient disturbance and of patient disturbance on staff disagreement. The research units are set up specifically for this study and function as therapeutic communities.

The EPPI Behavioral Disturbance Index, developed for this project, is used to determine the amount of disturbance the patient has shown during the preceding week. Staff attitudes are measured by a Patient Weekly Rating Form, filled out for each patient by each staff member. A measure of consensus is developed which is relatable to the quantity of patient disturbance.

It is found that there is a statistically significant reciprocal effect for the male patients, but not for the female patients, when high and low scores are compared on successive weeks.

Explanation of why the reciprocal effect does not show up with the female patients is sought; and after various alternatives are explored, the possibility of a second "reciprocal effect" is suggested, this being a putative failure in communication between staff and female patients.

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Tranquilizer Effects in the Social Status, Motivation, and Learning of Monkeys

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In that area of research which seeks to understand psychotropic drugs (especially tranquilizers) by utilizing animal behavior as a dependent variable, the scope of inquiry has been somewhat limited. Although there have been studies in which the subject is supposed to acquire a new response (e. g., avoiding a certain part of the apparatus), or in which he is allowed to perform a previously acquired response (e. g., lever pressing), a drug effect is usually not attributed to an influence on intellectual processes. Interest has focused on motivation and emotion, principally forms of motivation and emotion that are engendered by electric shock; and little attention has been directed to basic learning mechanisms. Another characteristic of this research area is the use of doses which if translated into the realm of human psychopharmacology would be considered massive.

Conditions which have fostered the tendencies just described are (1) the fact that tranquilizers came into (and now possess) prominence because of their ability to ameliorate disturbed and emotional patterns of behavior, (2) the "success" of investigations which use electric shock and large doses, and (3) the accumulation of data which indicate that basic learning processes, as traditionally studied, are not primarily affected by most of the drugs.

The present study employs a neglected method of examining drug effects in learning. Two discrimination tasks have been designed. The tasks are highly similar, if not identical because the basic unit in each task is the same: a pair of multidimensional objects, one object being correct (choice exposes a food reward) and the other object being incorrect. A serial presentation of pairs [13] has been combined in one task with contrasting conditions of reward and nonreward in preliminary presentations of single objects,[15] and, in the other task, with variation in the amount of reward bestowed on different pairs.[14] Analysis of choice behavior will disclose drug effects for reinforcement and nonreinforcement in several contexts. The general hypothesis underlying the use of these discrimination tasks is that tranquilizing drugs may selectively affect certain response patterns and central mechanisms which are involved in ordinary learning situations where frustration and fear, though possibly important, are not acute.

Another approach has also been utilized in the present study, namely, the social-dominance test. A social situation provides a wealth of semi-instinctual and learned behaviors, which, as Norton and De Beer [21] point out, may not be amenable to study by current conditioning techniques. One barrier to the use of animal interactions has been the difficulty in identifying reliable measures and in giving them a meaningful interpretation without the stain of anthropomorphism. The techniques and results of social dominance testing have been sufficiently worked out that this barrier may be penetrated to a limited extent.[19,26]

Since the discrimination tasks and the social-dominance test both utilize a food incentive, a separate test of motivation has

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been included. However, monkeys' performances might be presumed to be independent of degree of food deprivation according to published reports (e. g., Meyer [17])—unpublished work of one of us (R. W. I.) to the contrary.

The choice of chlorpromazine and meprobamate in the present study stems in part from a desire to compare two well-explored compounds which supposedly differ in the locus of their physiological activity,[2,6] and in part from the suggestive results of preliminary experiments. The dose levels are sufficiently light that the results may not reflect impairment of motor skill, suppression of sensory stimulation of the cortex, or induction of a stuporous state.

Method *

Subjects.—The subjects were six testwise young rhesus monkeys, whose weights ranged from approximately 3.2 to 4.2 kg.

Apparatus and Materials.—The Wisconsin General Test Apparatus (WGTA) was used in both discrimination and social-dominance tests. Essentially, it consists of a cage occupied by the animal (two animals for social-dominance tests); an opaque screen, just in front of the cage, which is lowered between trials; a sliding tray containing two depressions, and a one-way vision screen just in front of the experimenter. For discrimination experiments test objects were drawn from the laboratory's catalogue of over 400 randomly paired multidimensional objects. A portable weight-pulling apparatus, described elsewhere,[16] was available at the home cages.

Injection.—The subject, taken from his home cage in a transport cage, was introduced into a special holding cage made of two sets of interlacing bars. By moving the bars together, it was possible to immobilize the monkey. Chlorpromazine and meprobamate were administered by intramuscular injection, the doses being approximately 0.23 and 55.0 mg/kg., respectively.

Design and Procedure.—Three treatments were utilized: (1) Condition C, injection of chlorpromazine; (2) Condition M, injection of meprobamate, and (3) Condition N, normal condition. In Condition N there was no injection of saline, since work at this laboratory and elsewhere [24] has shown no difference in test behavior between monkeys receiving a control injection and monkeys remaining untreated.

Each condition lasted a week, actually five consecutive days, for there was no testing on Saturday and Sunday. Each animal was subjected to each condition twice, and the sequence of conditions was different for each animal.

During the first three weeks the following daily schedule was instituted: (1) injection at 1:40 p. m.; (2) social-dominance tests at 2:00 p. m.; (3) single-object serial discrimination at about 3:30 p. m.; (4) heterogeneous-reward serial discrimination at about 4:00 p. m., and (5) weight-pulling tests between 5:00 p. m. and 6:00 p. m. In the last three weeks the single-object discrimination and weight-pulling tests were abandoned, and heterogeneous-reward serial discrimination commenced approximately 30 minutes earlier.

In the social-dominance tests all possible pairs of monkeys were used each day, with the order of the 15 pairs randomly varied. A test consisted of 10 trials. A trial may be defined as the presentation of one piece of food on the test tray and prehension of this food by one of the monkeys. A trial began with raising the opaque screen and pushing forward the tray; it ended, about 10 seconds later, with the withdrawal of the tray and the lowering of the screen. Observations of behavior were made during the trial, which lasted about eight seconds, and also in the two- to five-second interval which came between trials.

In the single-object serial discrimination test [15] 12 objects were presented on the test tray singly and successively. Two complete runs were given through this "list"; then these objects were each paired with a new object. The list, now consisting of 12 pairs, was presented two times, and the subject was allowed one choice in each pair. Throughout the test six of the original singly presented objects were correct; and when the monkey moved them aside, he discovered food reward (raisin). The remaining six original objects were always incorrect. In each pair only one object was correct, the new objects in every pair being assigned the opposite value from the original object. New problems, i. e., new lists of objects, were employed every day. Each trial (presentation of a pair) required about eight seconds, and one trial followed another with only a brief pause, during which the opaque screen was lowered.

The basic procedure for heterogeneous-reward serial discrimination [14] was exactly like that used in single-object serial discrimination except that pairs were used on all runs and the reward was variable. The correct object in three randomly selected pairs had a two-peanut reward, and in the other four pairs, a half-peanut reward. A

* Riopelle and Pfeiffer [25] provide further descriptions of the kind of test apparatus (Wisconsin General Test Apparatus), the type of discrimination object, and the trial procedure which were used in the present study.

new seven-pair list was presented for six runs at each session.

In the weight-pulling test the subject had to grasp a handle on a test tray and pull it forward in order to obtain a food reward. Weights suspended from the tray were increased in a stepwise fashion until the monkey failed.

Results

Single-Object Serial Discrimination.—There was no indication of a drug effect in any response category.

Heterogeneous-Reward Serial Discrimination.—Error scores displayed in the accompanying Table reveal a large difference ($P<0.01$) between errors made on small-reward (half-peanut) pairs and on large-reward (two-peanut) pairs. This difference, which was anticipated (Leary [14]), made it reasonable to conduct separate analyses of variance for errors made on pairs with large-rewards, pairs with small-rewards, and all pairs.

There was a significant *treatment* effect for total errors (all pairs) ($P<0.05$) and for small-reward errors ($P<0.01$), but not for large-reward errors. The differences between the three treatment means were evaluated by *t*-tests. A significant difference was found between Condition M and Condition N for total errors ($P<0.02$) and for small-reward errors ($P<0.01$). The difference between Condition M and Condition C was significant ($P<0.02$) only in the case of small-reward errors. No significant difference was obtained between Condition C and Condition N in any of the three categories.

Weight-Pulling Test.—Each animal showed an improvement in weight pulling between the first and the third week, but at no time was there any indication of a deleterious drug effect. The mean maximal pull, in pounds of lifted weight, was 37.7 for Condition N, 38.1 for Condition M, and 38.4 for Condition C.

Food-Getting in Social-Dominance Tests. In any pair the dominant animal was defined as the one which obtained over 50% of the pieces of food. Rankings were made in terms of the number of animals which any given animal dominated. Previous tests had established a highly transitive rank order in the group of six monkeys. In the present study the pairings which were made with animals under identical conditions (giving additional weight to pairings in which both animals were undrugged) yielded the same rank order.

Percentage of Discrimination Errors, Total Pieces of Food, and Total Aggressive Acts

	Heterogeneous-Reward Learning		Social-Dominance Test	
	Half-Peanut	Two-Peanut	Food	Aggression
Condition N	16.6	6.4	1,552	603
Condition M	22.2	10.7	1,700	664
Condition C	19.1	8.7	1,248	187

Out of 450 pairings that occurred over the entire 30 days of testing there were 49 in which the previous or normal dominance relation was reversed. Eleven of these shifts occurred while the normally dominant animal was in Condition N, 11 while this animal was in Condition M, and 27 while this animal was in Condition C. There were only four instances in which a monkey slipped one or two ranks (never more than this) in his average ranking for a week. In three of these the monkey which slipped was under the influence of chlorpromazine. The remaining instance involved a normally dominant monkey in Condition N and a normally submissive animal in Condition M.

A similar picture is obtained from an examination of scores which show how many pieces of food each monkey gained in each condition. Condition totals (Table) reveal that chlorpromazine is associated with a decrease from the normal level and meprobamate with an increase. An analysis of variance yielded an F significant at the 0.01 level of confidence, and subsequent *t*-tests show that the mean score for Condition C was significantly lower than the mean scores for both Condition N ($P<0.02$) and

Condition M ($P<0.01$). The latter two conditions did not differ significantly.

Aggression in Social-Dominance Tests.—An analysis of variance performed on a number on aggressive acts failed to show that drug treatment had a significant effect despite large differences, which may be seen in the Table. Since there were tremendous individual differences which caused the error variance to be inflated, and since a drug effect was obvious in some individual monkeys, a different statistical procedure was employed. χ^2-Tests were calculated for the individual distributions of aggressive acts among the three conditions. Five of the monkeys had minimal scores in Condition C, and for three of these the χ^2 was significant at the 0.001 level of confidence; one had a χ^2 significant at the 0.01 level, and one had a χ^2 lacking in statistical significance. The sixth monkey exhibited a different tendency. This subject, which was next to the bottom of the dominance hierarchy, had scores of 1, 4, and 3 for Condition N, Condition C, and Condition M, respectively. Tentatively, we conclude that individual monkeys will lose much of their aggression under the influence of chlorpromazine (not meprobamate).

It should be noted that under normal conditions the dominant monkey in a pair attacks the submissive monkey more than he is himself attacked. This rule held in the present study except where reversals of normal dominance occurred; then the normally dominant animal tended to retain the prerogative for attacking. However, during such reversals the aggression was slight, and disappeared altogether when the now-dominant monkey obtained 90% to 100% of the pieces of food.

Comment

There is little indication in the research literature that tranquilizers affect the intellectual ability of man (e. g., Kornetsky, Humphries, and Evarts[12]; Kelly et al.[11]; Primac and associates[23]) or of monkey (e. g., Riopelle and Pfeiffer[24]; Hall and associates[5]; Pfeiffer et al.[22]). A drug-produced decrement in learning, problem solving, or performance of a skill can usually be attributed to a nonintellectual factor, such as decrease in motor coordination or block of sensory input. In light of this evidence, the present finding that chlorpromazine was without significant influence in discrimination learning is not surprising. However, in an earlier, unpublished experiment, one of us (R. W. L.) found that promazine injection (30 mg/kg.) produced an increase in errors in single-object serial discrimination, principally in pairs containing an originally unrewarded object; and a decrease in errors in heterogeneous-reward serial discrimination, principally in pairs in which the reward is small. It does not seem likely that such similar drugs as chlorpromazine and promazine should have dissimilar effects in learning, and the possibility exists that the discrepancy is explicable in terms of some detail of experimental procedure.

The evidence seems very clear that meprobamate spares the intellect,[11] and in a therapeutic dose does not weaken motor coordination.[10] Furthermore, it does not affect hypothalamic and autonomic functions,[2,6] or conditional avoidance response[2,22] (as chlorpromazine does), although it does attenuate emotional arousal in rats with septal lesions[9] and imprinting in mallard ducklings.[8] In the present study, monkeys treated with meprobamate made significantly more errors in heterogeneous-reward serial discrimination than when they were undrugged. Since there was no effect in the other discrimination task, despite their fundamental similarity, it may be concluded that "higher mental processes" are not impaired by this drug. The difference between the single-object problem and the heterogeneous-reward problem appears to be crucial. In the former problem monkeys show greater accuracy in pairs containing originally unrewarded objects than in pairs containing originally rewarded objects, but the level of proficiency is generally high.[15]

On the other hand, relatively great differences in accuracy are observed in the heterogeneous-reward problems between large-reward pairs and small-reward pairs; the level of proficiency is generally low.[14] One possible explanation is that performance is affected by a frustrational (emotional) response. Perhaps meprobamate facilitates this response; but since performance is still selective with respect to amount of reward, there is certainly no really disruptive emotionality. It is possible that the "stress" engendered by the heterogeneous-reward problem potentiates the effect of meprobamate on a reinforcing mechanism. Evidence for such an explanatory approach is found in investigations of "reward" and "punishment" centers in the brain,[3,27] and in the discovery by Kirschner (cited by Miller[18]) of differential drug effects on responses which switch on and switch off hypothalamic stimulation.

The principal results in the social-dominance test were that chlorpromazine reduced both aggression and food-getting success and that meprobamate had no significant influence on behavior. In all animals, differences between Condition N and Condition C were relatively larger for aggression than for food getting. For example, one monkey had 88 instances of hitting and biting and got 45 pieces of food in Condition N, whereas his scores in Condition C were 12 and 44, respectively. The implication is that aggression and food-getting possess considerable independence and do not always vary concomitantly.

Using a comparable dosage of chlorpromazine (0.25 mg/kg.) with two monkeys, Norton[20] found a change in several categories of behavior which were derived from component behaviors in a check list. He noted increases in sociability, contentment, and excitement and a decrease in hostility. Since Norton's monkeys were in individual cages and social interaction was between monkey and experimenter, a direct comparison with our results is not permissible. However, an adaptation of his categories to the social-dominance situation allows these estimates for the effect of chlorpromazine: a decrease in sociability, hostility, and excitement, and an increase in contentment (inactivity).

At the dose levels employed, chlorpromazine had a much more pronounced influence than did meprobamate. This finding is consonant with clinical observation[1] and with the results obtained in a study of activity level in monkeys.[4] Hendley and associates[7] have reported that chlorpromazine (20 to 30 mg/kg.) produced a seemingly catatonic and generally unresponsive state in monkeys, although the drugged animals remained hostile and might attack the experimenter. On the other hand, meprobamate (250 to 400 mg/kg. orally) left monkeys alert, with little fear and without hostility. The present results show that at considerably lower dose levels chlorpromazine produces a qualitatively similar, though less marked, effect and meprobamate produces hardly any change. There was just enough evidence of a "bolder" or "carefree" attitude in the case of meprobamate to suggest a need for further investigation.

It is clear that drugged monkeys were at no disadvantage in the weight-pulling test. They appeared to possess normal motor ability, and we may conclude that they were fully motivated. Caution must be exercised in making inferences from the tests which occurred earlier and in which smaller food incentives were used. The social-dominance data furnish support for the hypothesis that chlorpromazine decreases motivation for food (Schmidt and Van Meter[25]). It should not be surprising to find that in monkeys the motivational effect of a drug is a function of the method of measurement.[18]

Summary

Six young rhesus monkeys were given four tests each afternoon following one of these three conditions, each of which lasted a week: Condition N (no injection), Condition M (injection of 55.0 mg/kg. of meprobamate), Condition C (injection of 0.23 mg/kg. of chlorpromazine). Initially,

all possible pairs of animals were constituted in a random order, and each pair was administered a social-dominance test. The second test was single-object serial discrimination, in which 12 objects were presented singly, half correct and half incorrect, and were then presented paired with new objects. The third test was heterogeneous-reward serial discrimination, in which seven pairs of objects were presented for six runs, a two-peanut reward being used for three pairs and a half-peanut reward for four pairs. The final test required the monkey to pull a weighted tray in order to obtain a food incentive.

Each animal experienced each condition twice (in different weeks), and the order of conditions was different for each animal. At the end of the first three weeks, after each animal had experienced each condition once, the third and fourth tests were abandoned.

Weight-pulling and single-object serial discrimination tests revealed no drug effect.

For heterogeneous-reward serial discrimination, performance was impaired in Condition M but not in Condition C. In all conditions many fewer errors were made on two-peanut pairs than on half-peanut pairs. Food-getting and aggression diminished in Condition C but not in Condition M. However, the significant change in aggression was limited to four monkeys.

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Effects of Age, Education, and Brain Damage on a Learning Digit Symbol Test

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The test to be reported on in this paper was designed to evaluate certain assumptions about the effects of cortical brain damage in humans. It is hoped that eventually the degree of differentiation will be accurate enough to justify the use of the instrument as a diagnostic tool. Therefore a control group was used typical of the type of patient population from which the usual differential diagnosis is made.

The test was designed to sample four areas shown by animal and human experiments to be sensitive to organic brain damage. These are visual-motor speed, learning, memory, and flexibility.

Visual-motor speed, as measured by the digit symbol test, is an ability severely affected by factors interfering with intellectual efficiency. Busse, Barnes, Friedman, and Kelty (1956) showed this test to be most severely affected by the aging process. Cohen's (1957) factor analysis of the WAIS indicates that the digit symbol is a specific factor unrelated to the primary performance test factors of the WAIS (except in the 60- to 75-year-old group). His data also suggest that the digit symbol test has more specific variance and less loading with the G-factor than any other of the subtests. This type of test was chosen for these reasons.

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This work was done at the Veterans Administration Hospital, Durham, N. C., and at the Duke University Hospital.

Copies of the learning digit symbol test, along with instructions, are available to any investigator wishing to use this test for research purposes.

Studies with primates have indicated that the ability to learn new problems is profoundly affected by brain damage, the type of deficit being somewhat dependent upon location of damage (Harlow and Settlage, 1947; Riopelle, Alper, Strong, and Ades, 1953).

Although some tests purporting to measure or detect brain damage in humans make use of learning scores, generally this function is ignored. Since studies with repeated presentations of a digit symbol test (Westrope, 1953) have shown continuing improvement over seven or more trials, it is reasonable to assume that on this task the naive subject has much room for improvement. If learning is expressed as a difference in score between first trial and best trial, it becomes independent of the level of performance at which the subject operates and should therefore be able to contribute some unique variance. In the present test it was believed that four successive trials on the same test should be enough to show significant improvement upon performance.

Memory has often been cited as being sensitive to organic brain damage, and deficit in it is one of the behavioral symptoms of brain damage. The literature about memory, however, is contradictory primarily because the type of memory tested is seldom specified and the many methods of testing memory are not directly comparable. The digit-span test, for instance, has been shown (Heilbrun, 1958; Tolor, 1956; Yates, 1954) to be of questionable value and may well test attention span rather than memory. The Wechsler Memory Scale, composed of many types of memory content, has also been

shown to be of little value (Hall 1957; Yates, 1954). Incidental memory and delayed recall, on the other hand, have been demonstrated by several studies (Dencker and Löfving, 1958; Hanvik and Andersen, 1950; Tolor, 1956) to be sensitive to organic brain damage. For this reason the memory portion of the test to be described is essentially an incidental memory test, in which the subject gets a large amount of exposure to the stimulus to be recalled but is not aware that he should remember it.

Finally, the lack of flexibility in the organic patient has been of great interest to psychologists. This loss has many names, such as rigidity, loss of the abstract attitude, inability to shift sets, etc. Many tests purport to measure this, and the success or efficiency of these tests varies widely. Certain of these tests have, however, demonstrated a good ability to discriminate patients with organic disease from those with nonorganic disease. The Grassi block design test has done well in several studies (Grassi, 1953; Harris, 1955; Parsons, Stewart, and Arenberg, 1957; Ptaceks and Young, 1954). Grassi's definition of abstraction in the test is substituting one color for another color while doing the design. Whether this represents a true test of abstracting ability or merely adds another dimension of difficulty is an unanswered question. Although the Wiegl color-form test has not discriminated well (Parker, 1957; Yates, 1954), adaptations of this test, such as the Michigan card-scoring test (McMurray, 1954), have looked promising. Altrocchi and Rosenberg (1958) report good discrimination on a new test, the Yarcorsynski block technique; but whether the discrimination deficit lies in the difficulty of shifting from one concept to another or in a basic inability to form many grouping concepts is, at present, impossible to say.

In primates the ability to shift has been measured by teaching the animal to choose one member of a pair of objects and then suddenly shifting the reward to the previously nonrewarded object. Studies using this technique (Riopelle et al., 1953) have almost always shown a striking deficit in the organic group, in terms both of learning and of learning set formation.

In the present experiment, the rigidity factor was tested with a negative transfer situation, i. e., the learning of a new, incompatible response to an old stimulus. Rather than train S in one response and then require an opposite response (as was done in the primate experiments noted above), *S* was required to reverse an already well-learned motor response, writing the mirror images of certain letters.

The Test

Figure 1 shows the test blank, eight digits, each with a different associated symbol. Four of the symbols are geometric, and four are mirror images of capital letters. These were the only four letters in the alphabet that were found, in preliminary testing, to be difficult to execute. A test booklet was used consisting of four test blanks interspersed with four sheets of unmarked paper and a memory test blank at the end.

The order of the digits on the test blank was randomized, with two restrictions—all numbers appeared twice for each block of 16 squares, and no number was followed by itself.

Administration of Test.—The following instructions were given to the subject:

"Look at the numbers at the top of the page. They go from *1* to *8* in order. Underneath each number is a mark of some kind. Under the *1* are two straight lines (E pointed to each symbol as he mentioned it); under the *2* is a *B* written back-

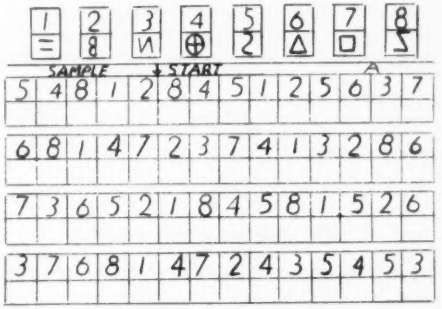

Fig. 1.—The Strong digit symbol test.

ward; under the *3* is an *N* written backward; under the *4* is a circle with a cross in it; under the *5*, an *S* written backward; under the *6*, a triangle; under the *7*, a square, and under the *8*, a *Z* written backward. Now these numbers down here (E pointed to the sample section) have no marks under them, and your job will be to put the mark in the box that goes with the number at the top of the page. Watch me." E then filled in the first two boxes of the sample and said: "Now you fill in the next three boxes and then stop." (If subject did this correctly, E was ready to proceed; if not, E made sure S understood by correcting his error and having him do part of the sample over again.) E then said: "Now, when I say 'Begin,' start here and fill in each box as fast as you can. Fill them in as you come to them without skipping any. At the end of one minute I will say 'Stop' and ask you to turn the page. The next page will be blank, and I will give you a minute's rest. Then we will turn the page and do the same thing over again for a minute. You will get four one-minute trials with a minute's rest between trials. Go as fast as you can and try to do better on each trial. If you make a mistake, leave it; don't correct it. Do you understand?" (If S indicated that he understood, E then said "Begin"; if not, E clarified any questions.)

At the end of one minute, E said: "Stop, turn the page."

Then, after 45 seconds of rest, E turned the page, drew a line through the sample section, encouraged S to do better, and said "Begin."

Immediately after the fourth trial E turned the page to memory page and said: "Now, let's see how many of these you can remember. If you aren't sure, please guess. This is not a speed test; so take your time." S usually needed about a minute, but was given as much as three minutes if needed.

The above instructions were followed as closely as possible; but if, in the examiner's opinion, changes were needed to assure comprehension, he made the necessary adaptations. S was never told in advance that there would be a memory trial.

Scoring for Test.—For each trial, the following items were scored: the number of correct substitutions, number of reversal errors, and number of other errors. Figure *2* gives examples of the commonest types of reversal errors. If S made a reversal error and then corrected it, it was still counted as a reversal error. E discouraged any correction attempts. Other errors included incorrect substitutions, grossly inaccurate figures, and writing in numbers. All of these were rare, and almost all errors were reversal ones.

Performance on the whole test was scored as follows: (*a*) maximum correct on best trial; (*b*) learning—difference between number correct on best trial and number correct on first trial—(*c*) memory—number correctly reproduced in memory section—(*d*) reversal errors. If a reversed letter was associated with the right digit but was not properly reversed, it was scored as an error.

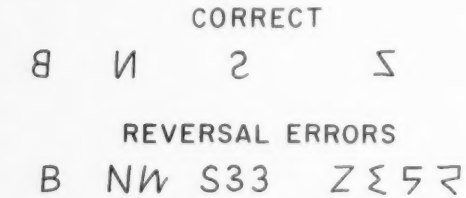

Fig. 2.—Common reversal errors.

Subjects

Three groups of subjects received the test, two patient groups, with and without cortical brain lesions, matched for age and education, and 53 college sophomores.

Lesion Group.—This group consisted of 36 patients of the Veterans Administration Hospital at Durham (an acute service, general medical and surgery hospital) and the Duke University brain tumor clinic, who had known cortical brain damage. No lesion patient was included in this study unless he was well oriented, had no gross motor or visual disturbance, and understood the test adequately. Table 1 gives the means and standard deviations of the ages and educations of all groups.

Nonlesion Group.—This group consisted of 35 psychiatric and neurological patients who had no evidence or suspicion of cortical brain damage. The disorders of these patients included schizophrenia, acute anxiety reactions, depressions, and severe psychoneuroses.

College Group.—This group consisted of 53 male and female students in an introductory psychology course. They are included to show the performance of above-average persons on this test. Most comparisons, however, will be between the first two groups. The test was administered individually to the patient groups, but was given as a group test to the college group.

Results

Table 1 gives the mean age, education, and test scores for the three groups used in this study. Reversed errors are determined by the following formula:

$$r = \frac{\text{No. of reversal errors}}{\frac{\text{No. correct}}{2} + \text{No. of reversal errors}}$$

It can be seen that this formula gives the number of reversal errors per opportunity

TABLE 1.—*Mean, Age, Education, and Scores for All Groups*

Group	No.	Age, Yr.	Education	Trials 1	2	3	4	Learning	Reversals	Memory
Nonlesion	35									
Mean		33.9	10.2	18.7	22.2	25.1	26.5	8.3	0.05	6.3
S. D.		7.4	3.0	6.2	7.6	7.9	7.9	3.8	0.05	1.9
Lesion	36									
Mean		36.9	10.8	14.5	16.7	18.1	18.8	4.8	0.11	4.6
S. D.		10.3	2.5	5.4	6.5	7.4	7.2	2.9	0.12	2.3
t(L-NL)		1.42	0.91	2.20	--	--	--	4.37	2.76	3.40
College	53									
Mean		--	14.0	31.8	37.2	38.7	41.4	10.1	0.07	7.3
S. D.		--	--	5.6	6.4	6.7	6.6	4.6	0.08	2.3

to make a reversal error and is therefore independent of general level of performance.

The *t*-test was applied to the differences between the means of the lesion and the means of the nonlesion patient group and are reported in Table 1. The difference on Trial 1 performance is significant below the 0.05 level, while the differences in learning, reversals, and memory are significant below the 0.01 level. It should be noted that learning and reversal errors are independent of the basic level of performance on this task.*

Table 2 shows the median splits for each of the four measures scored on the test. As can be seen, all four measures significantly separate the lesion from the nonlesion group.

Table 3 is an attempt to assess the diagnostic efficiency of the test by counting the number of patients in each group who failed at least three out of four measures or who failed two or less. A failure is counted as scoring below the median on learning, maximum performance, and memory, and scoring 0.10 or more on reversal errors. Using three or more failures as an indication of brain damage, it can be seen that 56% of the organic patients are correctly classified with a false-positive rate of 17%.

Such a method of cutoff scores, using the grand median of each score, is crude, and in a heterogeneous population such as this, in terms of age and education (and presumably intelligence), the nonlesion patients of low intelligence probably contribute significantly to the false-positive rate.

Figure 3 shows the mean number correct per trial for the lesion and for the nonlesion group, divided into a low-education (9th grade and less), and a high-education group (10th grade or greater). This graph shows the superiority of the nonlesion group. A particularly interesting point is the fact that the low-education nonlesion group starts at the same level as the high-

TABLE 2.—*Median Splits for Lesion and Nonlesion Group for Memory, Learning, Maximum Performance, and Number of Reversal Errors*

Measure	Group *	Split 5—	6+	χ^2	*P*
Memory	L	22	14	5.11	0.05
	NL	12	23		
		6—	7+		
Learning	L	22	14	5.11	0.05
	NL	12	23		
		22—	23+		
Maximal perform.	L	25	11	13.59	0.001
	NL	9	26		
		0.05+	0.04—		
Reversal errors	L	21	15	4.12	0.05
	NL	12	23		

* L indicates lesion group; NL, nonlesion group.

* The reader will note that the average difference between the means of Trial 1 and that of Trial 4 does not equal the mean learning scores. Since learning was defined as the difference in score between Trial 1 and the best trial, it can be seen that this might occur on Trial 3, Trial 4 being somewhat lower. Consequently, the mean learning score represents a maximal difference, not necessarily comparable to the difference between the means for Trial 1 and the means for Trial 4.

TABLE 3.—*Fourfold Table Showing Number in Each Group Scoring Below Cutoffs on Three or Four of Each Measure*

	Number of Measures Below Cutoff				
	3 & 4		2-0		
Group	No.	%	No.	%	χ^2
Lesion	20	56	16	44	11.32
Nonlesion	6	17	29	83	$P<0.01$

education lesion group, but shows greater learning over the four trials and ends at a higher level. Only two nonlesion patients failed all four measures, and both had an eighth-grade education or lower. Nine lesion patients failed all four measures, and six of these had a 10th-grade education or better. Not a single nonlesion patient whose education had progressed beyond high school fell in the organic cutoff range, while five of the lesion group correctly identified did progress beyond high school. On the other hand, 9 out of the 10 lesion patients who passed all, or all but one, of the four test measures had a 10th-grade education or higher. In the nonlesion group, on the other hand, 10 out of the 25 passing all, or all but one, of the measures had 9th-grade education or less. Thus, education, as a measure of intelligence, is related to performance, and cutoff scores for a diagnosis of organicity will have to take both age and education into account. Because of the limited size of the groups used here, however, it was felt that any scoring system weighted by these factors would prove unstable.

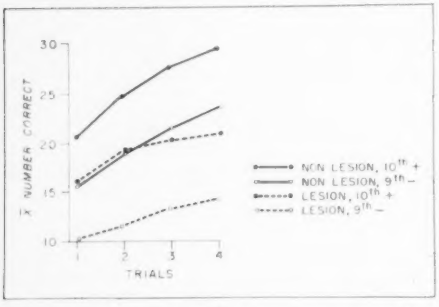

Fig. 3.—Mean number correct over the four trials as a function of education and brain damage.

TABLE 4.—*Correlations of Test Measures for the Lesion and the Nonlesion Group*

Measures	Lesion	Nonlesion
Learning and memory	0.20	0.11
Learning and maximum score	0.68 *	0.49 *
Learning and reversal errors	−0.29	−0.14
Learning and age	−0.13	−0.28
Learning and education	−0.02	0.42 †
Maximum score and memory	0.33 †	0.31
Maximum score and reversal errors	−0.45 *	−0.32 †
Maximum and age	−0.37 †	−0.52 *
Maximum and education	0.36 †	0.59 *
Memory and reversal errors	−0.21	0.01
Memory and age	−0.18	−0.30
Memory and education	0.27	0.07
Reversal errors and age	−0.14	−0.11
Reversal errors and education	−0.22	0.14

* Significant at 0.01 level.
† Significant at 0.05 level.

Table 4 gives the intercorrelations for test measures, age, and education. This Table, again, shows the importance of age and education on test scores and indicates that weighting for these factors, when a more adequate sample is obtained, should increase the diagnostic efficiency of the test. Maximum score obtained appears to be the best single indicator of test performance, and all other test scores are significantly correlated with this measure. It is interesting to note that organicity appears to disturb the correlations related to learning and education, indicating the ability of the test to identify brain damage in the high intelligence group.

Comment

In looking at the commonest types of reversal errors, it becomes evident why these three letters are the only ones for which the mirror-image reproduction is difficult. (The backward-written B was not at all difficult for most subjects.) The commonest error for the backward-written N (Fig. 2) was the W-type production; for the backward-written S the modified *3*, and for the backward-written Z was the production which looked like the Greek letter Σ (sigma). In all these errors it can be seen that if the first stroke is omitted, the remaining

production is strikingly like the correct or usual way of writing the letter. What seems to be occurring is that in the process of performing the new motor response the beginning of the old response is encountered, and this serves as a triggering mechanism to finish the total old response, thus giving an extra line to the *N* and *Z* and an extra loop to the *S*. This would seem to be a good example of what is meant by a defect of cortical inhibition in the organically brain-damaged patient. That is to say, he is unable to inhibit an old, well-learned response when it is now incorrect. Clinical observations of some of these patients bear this out. They would frequently make these mistakes even though conscious of them and vigorously trying not to make them.

Table 1 indicates that the college group clearly separates itself from the nonlesion patient group in terms of first-trial scores ($t=10.31$) and significantly, but not so sharply, on learning scores ($t=2.00$) and memory scores ($t=2.22$). None of the college group failed more than two of the four scoring categories. From these data it appears that this test discriminates over a wide range of age, education, and, presumably, intelligence. Preliminary tests with graduate students and medical students suggest that these groups rather clearly separate themselves from the undergraduate college group reported on in this study.

Certain other observations should be made about the test. First of all, most subjects taking the test seem to be well motivated by it and strive to improve their performance on each trial. Few seem to suspect that a memory test is coming at the end of the test.

Some preliminary work with children between the ages of 7 and 14 indicate that the test, although readily comprehensible even by brain-damaged children with IQ's as low as 55, is not useful for diagnosis, for two reasons: First, the motor writing habit is not well enough learned to make its reversal difficult; and, second, almost all children, even in the mentally defective range of intelligence, get almost perfect memory scores (7 or 8). Whether this is due to a high degree of eidetic imagery or to a relative lack of other interfering memories cannot, at present, be ascertained. It does seem probable, however, that the effective age at which this test will be useful is from 17 on. Upper limits are, as yet, undetermined.

Summary

A digit symbol test measuring visual-motor speed, learning, memory, and ability to shift a motor set was administered to two groups of patients, (with and without organic brain damage) and a group of college students. Significant differences were found on all four measures. A combination of these measures correctly identified 56% of the brain-damaged patients, while yielding 17% false-positive discriminations. It was also found that age and education affect these scores, and normative data for these variables are needed before the test attains high diagnostic efficiency.

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A Controlled Study of Efficacy of Iproniazid in Treatment of Depression

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By 1957 the ataractics or tranquilizers were widely used in the treatment of many mental diseases, including schizophrenia and the neuroses. There was, however, little change in the treatment of the depressive syndromes. The treatment of choice remained electroshock for the psychotic depressions. Psychotherapy was recommended for neurotic depressions, with the assistance of amphetamines for symptomatic relief. Thus the initial reports of Kline of the usefulness of iproniazid in depression in 1957 [1] were enthusiastically received, as it appeared that we could now extend the use of the psychopharmacologic agents into another type of mental illness, the depressions.

Iproniazid had been synthesized by Fox in 1951 [2] as part of a deliberate search for new antituberculosis drugs. It was first used in the treatment of tuberculosis by Selikoff, Robitzek, and Ornstein [3] and found to possess definite antituberculosis activity. In the same paper reporting this use, they also reported the drug to be a central nervous system stimulant. Their second paper on the drug reported the reversal of apathy and increased energy, suggesting that it might be used in the treatment of diseases other than tuberculosis. This conclusion was strengthened by the discovery of Zeller [4] that iproniazid is a strong inhibitor of monoamine oxidase, the primary agent in the metabolism of serotonin. The psychopharmacologic use of iproniazid was first reported by Jackson Smith [5] and by Kamman et al.[6] Smith administered 2 mg/kg. of iproniazid for a period of two to three weeks to a group of 11 patients encompassing several diagnostic categories and reached the conclusion that there was no evidence of marked or sustained improvement. Kamman and his group gave the drug to a group of 30 regressed, apathetic patients, a placebo to another, similar group, and no treatment to a third group, in a fairly well-controlled study. In terms of the ratings used, there was at no time any significant differences among the three groups, although some differences noted on the before- and after-treatment rating status of the iproniazid group made Kamman conclude that "something must be occurring as a result of the drug, although this 'something' is not very much."

There appears to have been little further interest in the use of iproniazid in psychiatry from the time when these papers were published, in 1953, until that of Kline,[1] in 1957. Kline reported on the use of iproniazid in an uncontrolled study on 20 chronically ill female patients selected by the criteria that they were withdrawn, regressed, deteriorated, colorless, with flattened affect, and in 9 patients seen in private practice, also selected on the basis of showing symptoms of withdrawal or depression. He concluded that at the five-month survey a minimum of 70% showed measurable response, and that if the organic cases were eliminated the response would have been 86% favorable.

After the initial positive report by Kline the drug began to be widely used in the treatment of depression, and numerous reports began to appear in the literature,[7-10] almost all reporting favorably on the drug, with some concerned about the side-

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effects.[9-11] Iproniazid had previously been replaced by isoniazid in the treatment of tuberculosis because of its many side-effects at the dosage required for antituberculosis activity.[12-14] Even at the initially recommended dosage of 150 mg. per day for psychiatric patients, side-effects became so prominent that the pharmaceutical company, producers of the medicament, revised their dosage recommendations in the spring of 1958, recommending a considerably lower dose than that previously used.

Because of the increasing interest in iproniazid in the treatment of depression, and the somewhat conflicting claims in the literature, we decided to undertake a controlled study to assess further the usefulness of the drug, comparing iproniazid with both psychotherapy and an inert placebo. As the criterion for the patients to be included in the study, we decided upon the single symptom of depression present to the degree that it would be included as part of the admission diagnosis. We thus had patients of all different diagnostic categories, including most of the psychoses, the neuroses, and some personality disorders with depression. It might have been preferable to have restricted our study to one diagnostic category only, but to obtain a sizable group in this fashion in this particular hospital would have been an extremely long procedure. We also had some justification for our selection in that most of the reported studies showed that iproniazid was of value in both the neurotic and the psychotic depressions.[15,16]

Method

All patients admitted to the hospital were screened by one of us, and those patients fitting the criterion for selection were placed on the study project and assigned a number. This number was given to the project psychologist, who then assigned this number to one of the three treatment groups, iproniazid,* placebo, or psychotherapy, on the basis of consecutive selection from a table of random numbers, thus securing random placement of subjects. For those patients assigned to the iproniazid or placebo group, a bottle of medicine containing enough for six weeks' treatment was made up by one of the hospital psychiatrists not connected with the treatment or rating of the patients, and sent to the ward with nothing on it but the name of the patient and the title "Special Medication."

After selection and assignment to one of the treatment groups, the patient was then rated on the Lorr[17] Multidimensional Scale for Rating Psychiatric Patients (MSRPP) by the ward administrator, the ward nurse, the resident therapist, and an independent rater not in contact with the hospital patient group, who presumably did not know to which treatment group the patient was assigned. Additional ratings were made at three- and six-week intervals by the same group of raters. In addition to the MSRPP ratings, the patient was given an MMPI test during the week prior to starting treatment and at the conclusion of treatment.

Treatment was begun exactly one week after the patient's admission to the hospital. Those patients assigned to the psychotherapy group received psychotherapy three times a week, of a nature designated by their particular therapist, and did not receive any drug, with the possible exception of sleeping medication. Those patients assigned to the iproniazid or placebo group received three 50 mg. tablets of iproniazid daily or three identical tablets of placebo. Treatment was continued for a period of six weeks unless the patient was removed from the project for one of the following reasons:

1. Opinion of the ward administrator that continuance on the project treatment constituted a hazard to the patient's health
2. Side-effects sufficient to necessitate discontinuance of medication
3. Termination of the treatment by the patient by either going AWOL or turning in a notice requesting release from the hospital
4. Improvement sufficient for discharge before conclusion of the six-weeks' treatment period (These patients were, however, rated at the time of discharge and included in the study.)

In addition to the assigned treatment, the patient was given privileges, such as group walks, campus privileges, and home visits, at the discretion of his individual therapist. In view of the novelty of the drug and the possibility of adverse reactions, a weekly complete blood count was performed on both the iproniazid and the placebo group.

It might be well to interject a note here about the actual treatment setting itself. The Columbus Psychiatric Institute and Hospital is a modern 126-bed hospital designed for the early treatment of acute mental disorders. All types of disorders are admitted, with the result that there are representatives of all diagnostic categories. The chief emphasis is on treatability; so most of the admis-

* Iproniazid (Marsilid) and identical placebo were supplied by Hoffmann-La Roche, Inc., Nutley, N. J.

TABLE 1.—*Composition of Treatment Groups*

Group	No. of Patients	Male	Female	Average Age, Yr.
Iproniazid	28	11	17	50
Placebo	30	6	24	47
Psychotherapy	31	10	21	53

sions are first admissions. Almost all patients in the hospital are voluntary patients who sign themselves into the hospital, and only voluntary patients were accepted for the study.

Results

A total of 89 patients were accepted as meeting the criteria that we had imposed and were included in the study between the dates of Aug. 5, 1957, and May 17, 1958. According to our randomization procedure, these were distributed among the three groups as shown in Table 1.

A comparison of the number assigned to the various groups and the number actually completing treatment is shown in Table 2. A breakdown on the causes of failure of those patients who failed to complete the full six weeks of therapy is shown in Table 3.

The lost or invalid category contains those patients who were discharged before a final independent evaluation could be obtained on them, or those patients who were evaluated by some one other than the independent rater, when, for example, he was ill or on vacation. We had originally felt that some one other than the independent rater could

TABLE 3.—*Causes of Lack of Completion of Six Weeks' Therapy, and Distribution*

Reason for Failure to Complete Therapy	No. of Patients		
	Iproniazid	Placebo	Psychotherapy
1. Side-effects of the medication	4		
2. Contraindications to medication	2		
3. AWOL or AMA*	3	3	2
4. Unmanageable on project treatment	2	7	7
5. Lost or invalid	2	4	3
	13	14	12

* AMA indicates discharge against medical advice.

TABLE 2.—*Comparison of the Number of Patients Assigned to Each Group, and the Number Completing Six Weeks' Therapy*

Group	No. of Patients Assigned	No. Completing Six Weeks' Therapy
Iproniazid	28	15
Placebo	30	16
Psychotherapy	31	19

make evaluations when he was absent provided the person was not connected with the treatment of the patients, and was blind as to what group the patient was assigned. However, subsequent analysis showed such a low correlation among raters that we could not make substitutions, and thus these patients were lost to the project.

It will be noted from Table 3 that there were approximately the same number lost from each group, but that the iproniazid group had a loss of six patients for side-effects and contraindications, which the other groups did not have, and it also had a somewhat lower drop-out rate for unmanageability.

The side-effects requiring cessation of therapy are reported in Table 4. There were two patients who were dropped from the project for contraindications to the medication that turned up after treatment was started. One was a patient whose treatment was discontinued because of severe renal impairment, and the other was a patient whose therapy was discontinued when he was found to have metastatic lesions in the central nervous system.

The MSRPP was scored for each patient examined by the independent rater. The nursing portions of the scale were not used, as the nurse was not completely ignorant of

TABLE 4.—*Side-Effects of Iproniazid Requiring Cessation of Therapy*

No. of Patients	Side-Effects
2	Peripheral neuritis
1	Convulsions
1	Severe edema

TABLE 5.—*Initial and Final MSRPP Morbidity Ratings, and Change During Treatment*

Group	Average Initial Rating	Average Final Rating	Average Change of Morbidity Scores
Iproniazid	30.7	22.9	7.8
Placebo	26.9	21.7	5.2
Psychotherapy	27.8	18.6	9.2

the treatment used, knowing whether the patient was receiving medication or psychotherapy. An over-all morbidity rating was obtained by subtracting the raters' response on each item from the normal response listed in the *Veterans Administration Technical Bulletin* describing the scale, and then adding the absolute values of these differences of each item. An attempt was made to use the depressive factor of the scale, since the criterion for selection and treatment was that of depression, but this could not be used, as the relatively small number of items, and the bipolarity of the items, made the net difference of before- and after-treatment scores zero in almost all cases.

Utilizing the morbidity scores for the independent rater for those patients completing the full six weeks of therapy, we find the averages as reported in Table 5.

As noted in Table 5, the psychotherapy group showed numerically the greatest change, the placebo group showing the least and the iproniazid group falling between. Because of the lack of data to show that the MSRPP data are interval in nature, a nonparametric test, the Mann-Whitney U-Test, was chosen to test for any significant differences. Utilizing this test, there are no

TABLE 7.—*Comparison of Ages of Those Patients Completing and Not Completing Six Weeks Therapy*

Group	Average Age of Those Completing Six Weeks' Therapy	Average Age of Those Not Completing Six Weeks' Therapy
Iproniazid	50	51
Placebo	47	47
Psychotherapy	52	54

TABLE 6.—*Average Score of Patients Not Completing Six Weeks' Treatment, Compared with Those Who Did So*

Group	Average Initial Score of Patients Not Completing Six Weeks' Treatment	Average Initial Score of Patients Completing Six Weeks' Treatment
Iproniazid	32.6	30.7
Placebo	32.2	26.9
Psychotherapy	30.6	27.8

significant differences among any of the three groups.

One of the problems of this study was the high drop-out rate. In order to test for the presence of any significant differences among the drop-out groups, or any bias involved in these patients dropped from the groups, a number of items were examined. The first comparison shown in Table 6 is that of the average scores of patients completing six weeks' therapy with those who did not do so. As noted above, there is little difference in the scores of the drop-outs of the different groups.

In order to see whether age was any factor in the drop-outs, the average ages of those completing and those not completing the six weeks of therapy were compared in Table 7. It would appear from this Table that age was not a factor in the scores of those patients dropped from the study. It has been suggested [14] that younger patients do better on iproniazid than do older patients. To test this, the improvement scores were correlated with the age of the patient, using the Spearman Rank Correlation Coefficient, and a correlation of 0.15 was obtained, which was not significant.

It was felt that there were insufficient numbers present to break the groups into

TABLE 8.—*Classification of Patients Completing Therapy on Basis of Diagnosis of Neurosis or Psychosis*

Group	Neurotic	Psychotic
Iproniazid	5	10
Placebo	6	10
Psychotherapy	5	14

TABLE 9.—*Classification of Patients Not Completing Six Weeks' Therapy on Basis of Diagnosis of Neurosis or Psychosis*

Group	Neurotic	Psychotic
Iproniazid	4	9
Placebo	4	10
Psychotherapy	5	7

diagnostic categories; so the patients were grouped as psychotic or neurotic, and the classification shown in Tables 8 and 9 was obtained. The composition of all groups, including those dropped before the prescribed six weeks of therapy, appears to be fairly constant, about one-third of each group being neurotic and the other two-thirds being psychotic.

In order to see whether iproniazid showed any particular efficacy in one group as compared with another, the changes of morbidity scores for the neurotic and psychotic patients for both iproniazid and placebo were compared, as shown in Table 10. It thus appears that in both groups the neurotic group improved somewhat better than the psychotic, but as there is no significant difference between the iproniazid and the placebo ratings, it would appear that iproniazid is little better in one group than in another.

The actual composition of the iproniazid and the placebo groups using the selector's diagnoses is shown in Table 11. The depressive and psychasthenic scales of the MMPI were tabulated and showed no significant differences as a result of treatment among the three groups, thus appearing to confirm results obtained by use of the MSRPP.

TABLE 10.—*Relationship of Diagnosis to Treatment*

Group	Average Change of Morbidity Scores (Improvement) During Treatment	
	Neurotic	Psychotic
Iproniazid	11	6
Placebo	10	4

Comment

One of the criticisms of this study might be our choice of depression as a symptom for our dependent variable. We have previously pointed out our reasons for our selection of this factor for study, and we realize that our data apply only to this factor. It is thus conceivable that a particular diagnostic group, such as patients with manic-depressive psychoses, or with involutional psychoses, might respond to the drug while other diagnostic groups might not, and that our study would not show this. We are forced to acknowledge this and can only wait for further studies to prove or disprove this point. We do, however, feel justified in our particular selection not only for the reasons previously given but also because of the fact that previous clinical studies have reported favorable results in most depressive categories.

TABLE 11.—*Composition of Iproniazid and Placebo Groups by Diagnosis*

Diagnostic Category	No. of Patients in Iproniazid Group	No. of Patients in Placebo Group
Neurotic depressive reaction	5	6
Manic-depressive reaction, depressed	1	1
Involutional reaction	5	2
Psychotic depressive reaction	3	4
Schizoaffective reaction	1	3

Another point worthy of discussion is that of the period of treatment used. Iproniazid is known to be a slow-acting drug, and it might be criticized that we utilized a treatment period of only six weeks. However, most investigators report that some degree of improvement is manifest in the first two to four weeks,[10,11,15] so that we should certainly be able to note improvement in a period of six weeks, even though the patient might not have reached maximum improvement by that time.

Probably the chief point of discussion of this paper concerns the large number dropped from the various groups for various reasons. It might be argued that the

large number dropped from the iproniazid group could obscure the effect of the iproniazid. Although there is no way of being completely certain of this, our analysis of the groups and the drop-outs on the basis of several factors would appear to indicate that there was no particular bias involved. Certainly, on the basis of the factors that we have analyzed, there appears to be no difference in any of the drop-out groups. One of the largest causes of loss was adverse reaction to medication, which might very well be construed as a failure in therapy.

Summary

Data are presented on a controlled study comparing iproniazid with both an inert placebo and with psychotherapy. Within the limitations of this study there is no difference in the efficacy of iproniazid, of an inert placebo, or of psychotherapy, in the treatment of depression as a symptom.

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Waiting for Hospitalization

A Study of Persons Placed on a Hospital Waiting List and of Their Families

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Introduction

Hospitalization for mental illness is a serious, and at times traumatic, event for both patients and their families. Once the family or the patient's physician has decided that the patient should be hospitalized, waiting for hospitalization may be disruptive of family and individual emotional health. A survey of studies [1,5] of hospital waiting lists reveals most of them to be unsatisfactory in terms of asking such questions as these: How does the family react to waiting-list placement? How does the potential patient react? What kinds of behaviors led to waiting-list placement? What factors contribute to the family's perception of the pre-waiting-list situation as being or not being a critical one? The present study is an attempt to explore these questions.*

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The study is part of the work being done at the Massachusetts Mental Health Center, Boston, by the Rehabilitation of the Mentally Ill Project, Office of Vocational Rehabilitation Grant No. 36-57-C1; and the Prevention of Hospitalization Project, National Institute of Mental Health Grant No. OM-67(R). The original research was carried out by M. T. Breslin, A. B. Gurewich, A. S. Evans, E. B. Sandler, and M. Stolof, under the direction of J. Mohr, Simmons College School of Social Work, and D. Landy, Boston University School of Social Work.

* We are speaking here specifically of studies of families of persons on hospital waiting lists. However, the impact of mental illness on the family, even after hospitalization, had not been systematically studied until the pioneering work by Clausen and his associates [2] in recent years.

This paper has as its main objective to describe the prospective patients, and to try to clarify the psychiatric and familial nature of persons placed on the waiting list in a small, short-term psychiatric hospital in order to understand some of the main cues that family or significant other persons might choose as indices of a person's need for hospitalization. It is our feeling that if these indices are more easily recognized, preventive psychiatry and rehabilitation would be greatly facilitated by moving into a case at a more strategic time. Usually patients are hospitalized during an acute episode of emotional distress. This means that the hospital perforce must first deal with the crisis situation before dealing with the more basic psychological problems underlying the crisis.

Method

Examination of the waiting list at the Massachusetts Mental Health Center in the winter of 1956 revealed that of approximately 100 persons listed, permission to interview families of 75 of these persons was given by the referring physician. Under the supervision of their research advisors, five social work graduate students interviewed these family members (one interview later was lost). We reanalyzed the raw data in an attempt to reassess and reevaluate the results.

Families were contacted first through securing permission of the referring agent and then through the permission of the family member or other relevant person. Initially it was hoped that the family would be interviewed before the prospective patient was hospitalized, but it was found that some had already been hospitalized at the Massa-

chusetts Mental Health Center or elsewhere by the time our interviewers had reached them. In the present sample all persons who had been on the waiting list at least one week were included. Also they were either living or in close contact with their immediate family. These cases undoubtedly did not have the same freshness of recall as the ones with less time lapse. A methodological limitation was that time would not permit more than a single interview per informant, even though for further exploration in depth additional interviews would have been desired. Nevertheless, we feel that the areas covered are ones not usually included in such studies and the data, though limited by the usual factors of selective recall and distortion, will be useful for clinicians, social workers, and administrators.

Characteristics of the Group

The 74 family informants who provided the data for the study divide as follows: 28 spouses, 24 parents, 11 siblings, 6 children, and 5 miscellaneous kinship categories.

The potential patient sample numbered 61 females and 13 males, a ratio of 5:1. There were 36 married and 27 single prepatients, a reversal of the usual trend in hospital populations. The remaining 11 were separated, divorced, or widowed. (The married or formerly married patients had on the average less than two children.) Religious affiliation indicated 38 Catholics, 22 Protestants, 11 Jews, and 3 Greek Orthodox. There was only one Negro prepatient in the sample. The age range was 16 to 60 years, a reflection, perhaps, of the fact that this hospital does not encourage the admission of patients older than 60; the median age was 36 years, the bulk of the group being under the age of 40.

Most of the prepatients (65%) had had at least some high-school training; 19%, some college education, and only 16% had never proceeded beyond grammar school. By occupation they were predominantly white-collar workers (31%) and housewives (35%); 11% were manual workers; 7% were students, 3% were professionals, and 12% were unemployed.

Economic roles in their families were as follows: essential breadwinner or contributor, 26%, and self-supporting, 14%; 60% were dependent on others (this includes 35% who were housewives, so that actually only 25% of the total potential wage-earners were not able, motivated, or willing to work). Another factor contributing to the economic complexion of the group is that the age of the patients was relatively low; for example, the 47 who were married or had been married would, of necessity, have children too young to be a support to them or to be left at home without their parents.

Most of these persons were living either with their spouse and/or children (46%) or with their parents (42%) at the time they became ill. Eleven per cent were living with other relatives or roommates; one lived alone. Thus, while these people may have been psychologically alienated from others, at least in terms of their residence patterns, they were not living alone.

About one-third (34%) had never been previously hospitalized for mental illness. Of the remainder, 30% had been hospitalized once before, 18% twice, 12% three times, 4% four times, and one each seven and eight times, respectively.† The mean number of previous hospitalizations for the group was 1.4 and the median 1.6, averages which indicate that on the whole the group had not been chronically ill enough to warrant extensive previous hospitalization. The mean length of previous hospitalization was 7.1 months and the median 3.3, with a range from a few days to as long as 48 months. The extreme variation (a few days to as long as 4 years) makes the median of 3.3 months a better estimate of the central tendency of the group. This reinforces the picture of a group which had not been pre-

† These findings reflect the decreasing proportion of first admissions to this hospital in recent years: 1955-1956, 51.9%; 1956-1957, 45.2%; 1957-1958, 44.3%.

viously hospitalized for long periods, though on the whole one could say that hospitalization for two-thirds of the sample would not be an unfamiliar experience.

Results

Referral to the Waiting List.—As the patient's symptoms became socially noticeable and seemed to require medical attention, 30% referred themselves to either the family doctor or a psychiatrist. The majority, 65%, were referred by a family member, and 5% were referred from miscellaneous sources. Thus, the possibility of hospitalization seems to become most real when the patient's family considers him ill rather than when he considers himself ill. When the family doctor or psychiatrist referred the patient to the hospital and he was placed on its waiting list, 64% of the patients knew of this referral, and 36% supposedly had no knowledge of this event. This is a rather surprising finding, for one might expect that most patients would be informed of such an important move. The fact that more than a third did not have knowledge of their possible hospitalization may indicate that a good deal of the early upset and turmoil that one observes in new patients entering the hospital may well be due as much to surprise and anger as to the uniqueness of the experience itself. This speculation is reinforced by the finding that almost all of those who knew they were placed on the waiting list consented: Of these, 55% knew of the impending hospitalization and consented to it; 36% did not know and therefore could not consent, and 9% who knew that their relatives were making such a move did *not* consent to it.

Duration of Symptoms.—An important aspect of any illness is the duration of symptoms which plague the patient and to which the family and others are exposed. The longer the symptomatology, one might predict, the greater the strain and the less the family's tolerance for the patient and his symptoms. We do not know how long these patients were actually ill (the onset of illness, as opposed to an observable acute episode, is probably in most cases so insidious and interrelated with other factors as to be indeterminable in any precise manner). Our sample, on the average, displayed noticeable symptoms for a relatively long time, showing a median of 2.2 years. Nevertheless, we find that a large number of respondents (32%) report symptoms of five years' duration or more, whereas an almost equal number (31%) report symptoms of one day to 5.9 months' duration. Symptom duration for the remaining 37% varied from 6 months to 4.9 years. This indicates that the duration of symptoms alone is not enough to predict whether a patient will hospitalize himself or be hospitalized by his family. Whether he will be hospitalized seems to depend more upon the ability of the family to tolerate his symptoms than their duration.

Reported Symptoms.—What are the behaviors and psychological states which the respondents say they observed in the patients? Table 1 shows the general categories of observed symptoms reported by our informants. The symptom most frequently reported was general depression. Then, in order of frequency, were reported bizarre

TABLE 1.—*Observed Symptom Classes Reported by Respondents (Multiple-Coded)*

Symptom Class	No.
Depression, including fear of dying, sense of failure and inadequacy, withdrawal, apathy, tiredness, inability to sleep	72
Bizarre behavior, including inappropriate responses, confusion, disorientation, phobias, illusions, and hallucinations	40
Anxious behavior, including agitation, nervousness, fearfulness, anxiety, compulsiveness	33
Hysterical behavior, including hypochondriasis, somatic complaints and complications, excessive crying, fainting spells	32
Paranoid behavior, including delusions and ideas of reference	19
Refusal or inability to work, including actual loss of job	17
Suicidal feelings or attempts	14
Socially bothersome behavior, including hypercriticalness, incessant demands, noisy activity, argumentativeness, narcissism, public exposure, and regression to previous psychotic pattern	13
Inability to care for self, including refusal or inability to eat or tend to bodily needs	10
Drug or alcohol addiction	3

behavior, anxious behavior, hysterical behavior, paranoid behavior, refusal or inability to work, suicidal impulses or attempts, socially bothersome behavior (a very general category into which many of the symptoms subsumed under other headings might also have been included), inability of the person to care for himself, and, finally, drug or alcohol addiction. In almost every case more than a single behavioral manifestation was reported as symptomatic of the patient's illness. The Table also illustrates the wide range of behaviors considered deviant enough to warrant the label or suspicion of mental illness.

Handling the Patient.—Table 2 demonstrates that the prepatients' families and friends tried in a number of ways to cope with the patient and his illness. In most instances some positive type of activity was undertaken to help the patient cope with his symptoms, and thus help the family itself better to withstand their impact. It appears that placing the person on the waiting list did not, in most instances, lessen other attempts to handle the patient; in fact, in many instances these attempts increased, as though placing the person on the list encouraged the family toward a hopeful attitude or expanded the limits of their tolerance. In several instances, however, perhaps because the patient's symptoms became exacerbated meanwhile, as well as because immediate hospitalization did not occur, the family felt impelled to try to place the patient in treatment or in another hospital. In some cases increased hostility toward the patient also occurred after referral to the waiting list, as though the family's limits of tolerance had been reached at the time of referral and they were unable to stem their anger after that time.

TABLE 2.—*Attempts of Family to Handle Patient Before and After Placement on Waiting List (Multiple Coded)*

Modes of Handling Patient	No. Before Placement on Waiting List	No. After Placement on Waiting List
Protecting the patient from harm, helping him in various ways, accompanying or staying with him, or getting others to stay with him	34	30
Actively reassuring, mollifying, humoring, agreeing with; use of calming tactics; submitting to demands	21	28
Took patient to private psychiatrist, clinic, other doctor, or rest home	16	18
Tried to persuade patient to change behavior, "straighten out"; discouraged hospitalization, talking patient "out of it"	9	14
Tried to ignore or withdraw from, remained unperturbed	7	7
Encouraged to perform usual occupational or avocational tasks	6	5
Became hostile toward, blamed, condemned	3	8
Tried to place patient in hospital or succeeded in doing so, called police, or tried to use other social controls	4	4
Encouraged patient to get help or treatment, prepared for hospitalization	1	3
Attempted to control behavior through drugs	3	2
Changed toward a more accepting mode of behavior	4	--
Tried to get friends to visit patients	1	--
Made arrangements to place children	1	1
Tried prayer	--	2

For most cases, however, there appears to be stability in the ways in which the prepatient is treated by his family or significant other persons from the time he is cited for hospitalization and during the interval after this, which varies from a few days to several months. Over half of our patients, 59%, were reported as being treated essentially the same before and after being placed on the list; an additional 11% were treated essentially the same, plus the putting forth by others of additional efforts to help; 7% reported changing their tactics in the direction of being generally more protective; 4% reported becoming generally more tolerant; 3% reported relief of tensions and preparation of the patient for hospitalization; 11% reported taking the patient to another hospital than the one to which they applied; 3% reported more withdrawal from the patient, and one said that the symptoms cleared to such an extent that no further care was necessary. (One did not report on this question.)

Changes in Residence.—Did the patient voluntarily or involuntarily change his residence as a result of discomfort or family

TABLE 3.—*Changes in Residence from Time of Perceived Onset of Illness to Time of Follow-Up Interview*

Residence at Time of Perceived Onset of Illness	Residence at Time of Follow-Up Interview	No.
With one or both parents	Institution	19
With spouse and children	Institution	15
With spouse only	Institution	1
With children only	Institution	1
With grandmother	Institution	1
With sister	Institution	2
With roommate or relative	Institution	2
With one or both parents	Parents	12
With spouse and children	Parents	1
With spouse and children	With spouse and children	10
With spouse only	With spouse only	6
With children only	With children only	1
With sister	With sister	1
With roommate	With roommate	1
Alone	Alone	1
	Total	74

and other pressures? From Table 3 it is apparent that more than half the sample, 55%, became institutionalized in our hospital or another by the time they were seen at the follow-up interview, regardless of their original domicile. Of the remaining 45%, only one prepatient changed residence, from conjugal to parental family.

The most stable living arrangement, or at least the one in which pressures seemed least likely to force the person placed on the waiting list either into a hospital or to a changed residence appears to be that of the childless nuclear family, since we found that six out of seven patients who were living alone with their spouses remained with them. Contrary to our expectations in the light of studies by Simmons and his associates,[3,4] the conjugal family which included children and the parental family appeared almost equally unable to tolerate the patient after he had been placed on the waiting list (63% of the first arrangement and 61% of the second). In other words, the least complicating living arrangement, and the one most conducive to circumventing hospitalization after the person has been deemed ill enough to be placed on the hospital's waiting list, appears to be the conjugal family sans children.

TABLE 4.—*Family Attitudes Toward Illness and Toward Hospitalization* *

	Accepts Illness	Rejects Illness	Total
Accepts hospitalization	31	17	48
Rejects hospitalization	2	21	23
Totals	33	38	71
		P=0.001	

* Three not ascertainable.

Family Attitudes Toward Illness and Toward Hospitalization.—To what extent did these potential patient families accept the patient's illness? How did they feel about possible hospitalization? From Table 4 it is apparent that, while nearly half the families accepted both the patient's being ill and the possibility of his hospitalization, nearly a third rejected both the reality of the patient's illness and the contingency of his being hospitalized. Thus, though in about one-third of the cases the psychiatrist or family doctor felt that the patient should be hospitalized, this possibility was not a welcome one to the patients' families.

This does not mean that in the event of hospitalization these families would not go along with the doctor's decision, but shows that they either held little hope that hospitalization would help or were actively opposed in principle to the idea. Many of these families, it should be emphasized, were also the most hostile toward the patient, and felt that his symptoms and the possibility that he might be institutionalized were both distasteful. Typical of this combination of attitudes is the instance of the potential patient's husband who felt that what troubled his wife was "definitely not mental illness, but [that] she's worried over the fact that she had abdominal surgery several months ago." This husband thought his wife should not go to a mental hospital, but, rather, that if she was to be hospitalized at all, it should be in a general hospital. He was also irritated by the fact that his wife was placed on a waiting list, since "people should go when they need it, especially to a state hospital, which is supported out of our taxes."

By contrast, let us take a case with a combination of accepting attitudes toward both illness and hospitalization. In this case the husband felt his wife's illness was "sickness, and I'm not ashamed of it." He had sought and received help from other agencies; and, since his wife had been hospitalized previously and helped by the treatment, he felt certain that the coming hospitalization would benefit her.

The two instances of families who accepted the illness but rejected hospitalization involved families who were deeply attached to the patients and feared that hospitalization might harm them. On the other hand, the nearly one-quarter of families who rejected the patient's sickness but accepted the idea of hospitalization were ones who appeared hostile toward the patient, frightened, or so anxious about what might happen (including the possibility of stigma against the family) that they welcomed hospitalization as a relief from their tensions, even though they did not necessarily feel that the process would actually benefit the sick family member. In a few of these cases the family actually felt that hospitalization "was just what he (she) deserves," indicating a punitive attitude toward the patient and a feeling that the hospital would be a punishing agent.

In summary, Table 4 shows that the two sets of attitudes are highly related to each other, and therefore somewhat predictive of each other. Whether the family has an accepting attitude toward the patient's illness or a rejecting one helps to determine how it will react toward the contingency of hospitalization.

Potential Patients' Reactions to Being Placed on Waiting List.—We have seen the attitudes of the families toward the family member's illness and possible hospitalization. How did potential patients react to being placed on the hospital's waiting list? We find that nearly a third were apprehensive about this possibility (Table 5), and many felt their families had deceived or tricked them, since they apparently had not been informed either by family or by physician. Another sizable group became depressed and/or disgusted with their families, and a similar-sized group knew nothing of the event or did not even realize they were considered ill. These were probably the sickest ones in the sample. However, a number equal to both these last two subgroups realized that they were ill and went along with the steps taken by the doctor and/or family. Interestingly enough, in 14 cases families seemed to know so little about the patients' reactions, or perhaps regarded this as of so little importance, that they were not able to report on this point in the interview.

TABLE 5.—*Patients' Reactions to Placement on Hospital Waiting List*

Reaction	No.
Potential patient became upset, anxious, frightened, bewildered, suspicious of family and/or doctor	22
Potential patient accepted event matter-of-factly, realized he was sick and needed help, accepted illness and hospitalization	16
Potential patient unaware he was ill, or of family placing him on waiting list, or denies illness	9
Potential patient felt ashamed, depressed, and/or disgusted with family	8
Potential patient accepted illness but not hospitalization	2
Potential patient reported to have had no reaction: acceptance or unacceptance of illness and hospitalization unclear	2
Potential patient experienced lessening of symptoms	1
Potential patient experienced exacerbation of symptoms	1
Potential patient felt he needed medication rather than hospitalization	1
Not ascertainable	14
Total	76

Family's Interpretation of Patient's Illness to Others.—The data in Table 6 show that about one-third of the families were able to use a relatively sophisticated explanation of why the individual behaved in the way he did and/or was being prepared for eventual hospitalization. We may conclude from this Table that many, perhaps the majority, of these families found it difficult to interpret the fact of mental illness of a family number to other people. This might be because of their own unwillingness to accept the fact and/or because they themselves may have been ignorant of all the implications of such an important event.

TABLE 6.—*Family's Interpretation of Illness to Others (Multiple Coded)*

Explanation or Interpretation	No.
Explained as having "nervous breakdown," "nervous trouble," "rundown condition," or by some other euphemism used	27
Frank, sophisticated explanation; patient was "emotionally ill," or "mentally ill"	23
Told only close friends or relatives of illness, no others	11
No interpretations; told no one	9
Explained strange behavior as due to reaction to drugs, to organic difficulty, or to other nonpsychiatric agent	5
Prepared others for possible "strange" or "unusual" behavior by patient; no other explanation	2
Frank explanation to some (e. g., adults); false explanation to others (e. g., children)	2
When hospitalized, explained patient's absence as being somewhere other than hospital	2
Not ascertainable	1

We do not know that all these families actually employed euphemisms deliberately; in fact, it is quite likely that for those families who may have accepted the illness but who did not have a certain degree of "sophisticated" (i. e., middle-class) knowledge, terms like "nervous breakdown" may have been as validly descriptive of what was happening as "emotional illness" was for families from other subcultural groupings.

Perception of Situation Leading to Waiting-List Placement.—An important antecedent of hospitalization is the family's perception of the situation. Exactly half of the families perceived the series of events leading to placement of the potential patient on the waiting list as a *crisis* situation, whereas almost all of the other half did not perceive it as a crisis (this variable was not ascertainable for three families). Statistical tests of significance (χ^2) showed the following relationships between perception of crises and other variables:

No relationship occurs between perception of the situation as a crisis and the number of previous hospitalizations of the potential patient, although one might have expected that the more the person had previously been hospitalized the more experienced the family might be with his behavior and the less likely they would be to perceive a situation arising out of such behavior as a crisis one. This lack of a significant relationship with crisis perception was found also with the sex of the patient, his or her age, and education (though some trend exists for those with some college training to be in families where the situation was not perceived as crisis as compared with those of less education).

There is a small, but nonsignificant, positive relationship between the family's perception of the situation as crisis and not obtaining the patient's consent to the waiting list, an expected outcome. There is also some positive relationship between the greater perception of the situation as crisis and patients living with conjugal families. This, too, is an expected result, since the conjugal family would be more apt to feel keenly the incapacitation or complete absence of a wife-mother or breadwinner. Lastly, we found practically no relationship between perception of crisis and whether the referring agent to a doctor or psychiatrist (prior to placement on the waiting list) was the affected person himself or his family.

Table 7 shows a marked tendency, barely short of significance, for families in which the potential patient was in an essential breadwinning role or in a self-supporting role to perceive the situation as a crisis, as opposed to families in which the affected person was merely an economic contributor or an outright dependent. It seems more likely that possibility of losing, or actually losing, temporarily or permanently, the economic returns of such family members

TABLE 7.—*Perception of the Situation Leading to Waiting-List Placement Compared with Economic Role of the Potential Patient*

Economic Role of Potential Patient	Perceived as Crisis	Not Perceived as Crisis	Total
Essential	5	3	8
Contributing	3	6	9
Self-supporting	8	2	10
Dependent	18	26	44
Total	34	37	71

Not ascertainable: 3

TABLE 8.—*Perception of Situation Leading to Waiting-List Placement Compared with Duration of Symptoms of Potential Patient*

Duration of Symptoms	Perceived as Crisis	Not Perceived as Crisis	Total
0-5.9 mo.	9	12	21
6-11.9 mo.	2	1	3
1 yr.-2.9 yr.	9	6	15
3 yr.-4.9 yr.	7	1	8
5 yr. or more	7	17	24
Total	34	37	71

Not ascertainable: 3

TABLE 9.—*Perception of Situation Leading to Waiting-List Placement in Relation to Whether Patient Was Overtly Aggressive*

Was Patient Overtly Aggressive?	Perceived as Crisis	Not Perceived as Crisis	Total
Yes	16	3	19
No	18	34	52
Total	34	37	71

Not ascertainable: 3 $P=0.001$

is coincident with greater emotional stress in the family. Their contribution to the general family budget is more apt to be missed and to add to the strains which already beset the family by virtue of the patient's deviant behavior.

There are positive, though nonsignificant, trends in the direction of more frequent perception of the situation leading to waiting-list placement as a critical one in families in which the troubled member was beset by his symptoms between 6 months and 4.9 years, and a tendency in the opposite direction for families in which the potential patient had exhibited his symptoms less than 6 months, or, at the other extreme, for 5 years or more (Table 8). This seems to indicate that the most critical time for the family is either in the early stages of symptom formation and display, before the family has become experienced with and used to them, or after several years, when the family's tolerance has been stretched to its limits. In fact, almost two-thirds of the patients, 63%, are in these two categories.

It is logical to ask whether perception of crisis is related to the symptomatology of the ill family member. We compared the variable with three sets of symptoms: those in which overt hostility and aggression were evident, including actual attempts at suicide; those in which the patient was depressed, including suicidal thoughts but not overt attempts, and those in which the patient was withdrawn. We found a highly significant relationship between perception by the family of a crisis situation and aggressive behavior on the part of the patient (Table 9). Interestingly enough, we did not find any relationship between perception of crisis and either depressive or withdrawal behavior, indicating that crisis perception may well be a function of how threatening and upsetting the patient's illness is to others, rather than to himself.

It was believed that the perception of crisis might depend ultimately upon which family member or other person was reporting the patient's behavior. We checked this variable against the identity of the informant and found almost no difference, with the exception of husbands. In the case of husbands, almost three times as many perceived no crisis as those who did. This reinforces our earlier finding that the conjugal family without children was the arrangement most conducive to tolerating the patient's behavior.

Families' Use of Community Resources.

After placing the patient on the waiting list, 42 (57%) of the families reported using community resources, other than the hospital, in dealing with the situation of illness in the family. The types and frequency of such use are detailed in Table 10; some of these families used more than one resource. Of those using such resources, 57% found them definitely helpful, 33% felt they did not help (but did not hinder) them, 5% felt they were definitely harmful, and one family each felt either that the resource was helpful but delayed its effectiveness or

TABLE 10.—*Utilization of Community Resources by Families After Waiting-List Placement (Multiple Coded)*

Use of Community Resource	Total
Used private hospital briefly	11
Referred patient to private psychiatrist	9
Talked with minister, priest, or rabbi	9
Used family physician or physical specialist	6
Sought admission at another state hospital	5
Used family service of other community clinic	4
Applied for help to department of public welfare	3
Applied for family service homemaker service	2
Applied for aid to dependent children	2
Placed children in foster home	2
Hired housekeeper to help	1
Sought help from social worker	1
Applied for veterans' disability pension	1
Known to have used no community service (other than waiting list)	30
Not ascertainable	15

that it would have been helpful if used. It is worth noting that clergymen were used about as often as private psychiatrists, but it should be clear that this might have been seen in either instance as a trend toward emergency or "stop-gap" use of most of these agencies while awaiting possible hospitalization of the sick person. We might conclude that better than half the families whose members had been placed on the waiting list were moved to use other resources rather than simply "wait out" the waiting period, though, in the sense mentioned above, use of such resources may have enabled them to "wait out" this crucial period.

Summary and Conclusions

A systematic study was made of 74 persons and their families who had been placed on the waiting list of a small, short-term treatment hospital. The most interested family members or other relevant person was interviewed on a series of variables relating to placement on the hospital waiting list.

Among the potential patients, there were five females to every male, four married persons to every three single persons, and an additional group of formerly married persons. The Catholic-Protestant-Jewish ratio was about 4:2:1, and all potential patients but one were white. The median age was 36 years, and most were under 40. The majority had had some high-school training and were predominantly housewives and white-collar workers. Economic roles in the family as essential breadwinner or contributor, self-supporting members, and dependents were in the ratio of about 3:1:6. At the time they became ill most patients were living in their conjugal or parental homes. Two-thirds of the sample had been previously hospitalized an average of 1.6 times for a total average of 3.3 months.

The majority were referred by a family member to a doctor or psychiatrist, who then placed them on the waiting list, indicating that only a minority voluntarily move toward hospitalization when they become—or are considered by others to be—ill. About two-thirds knew they were being placed on the waiting list, but the fact that a third did not demonstrates that a sizable number are referred for hospitalization without sufficient psychological preparation, thus adding to their confusion and anger upon eventually finding themselves institutionalized. Most of those who did know of the placement consented to it.

While symptoms had proceeded from a few days to several years, the median duration was 2.2 years, indicating that many families are able to tolerate unusual behavior for a considerable time. An expected wide and varied range of symptoms was reported by these families, in the following general order of frequency: general depression, various forms of bizarre behavior, anxiety, hysteria, paranoia, lethargy and lack of motivation, suicidal impulses or attempts, socially troublesome actions, helplessness, and drug addiction.

In most instances attempts of some sort were made by the families to deal with the patient's symptoms in the social situation; placing the patient on the waiting list frequently increased the number of actions taken by the family toward this end, possibly because the event gave the family some hope that a concrete step toward dealing with the problem was being taken.

Sometimes the family did not seem able or willing to wait for hospitalization at our institution and sought hospitalization elsewhere. In some cases hostility toward the prepatient increased. Most patients were treated pretty much the same before and after being placed on the list. More than half were hospitalized in our hospital or elsewhere by the time we had reached them for the follow-up interview. After the patient was placed on the waiting list, the conjugal family without children was much abler than either the conjugal family with children or the parental family to retain the patient in the family and, in effect, to delay or circumvent his hospitalization.

Almost half the families accepted both the patient's illness and the possibility of hospitalization, while nearly a third rejected both illness and hospitalization, despite the fact that the doctor or psychiatrist felt that the patient's condition warranted institutionalization. While many of the latter families were among the most hostile toward the sick family member, this finding should not imply that when the hospital admission became a reality they would object to it. In fact, a sizable number who were also rejective of the patient's illness accepted that he might be hospitalized, undoubtedly seeing this move as a way of helping both their own dilemma and the patient's. These results indicate that there is a high probability that acceptance or rejection of the family member's illness is correlated with acceptance or rejection of hospitalization.

These potential patients did not as a whole easily welcome the possibility that they might be hospitalized, only about one-fourth accepting the idea or experiencing a lessening of symptoms afterward. Most were upset and anxious and became suspicious of and disappointed in their families for permitting this to occur. Several were unaware that they had even been placed on the list, or their reaction was unnoticed or unreported by their families. Furthermore, only about one-third of the families were able to explain frankly to others the sick member's behavior as mental illness, while more than half explained the phenomena as "nervous breakdown," or through some other circumlocution.

More than half of the families were able to make use of community resources other than the hospital during the waiting period, and more than half of those who did found such resources to be helpful. These resources included a wide spectrum of agents and agencies in the community: clergymen, psychiatrists, and other physicians; brief stays in private hospitals; public and private welfare and social work agencies, and others. Probably if knowledge of such resources were more widespread, even more families would have availed themselves of such assistance, since it was obvious that for many the waiting period was a trying time. In fact, half the families felt that the situation leading up to placement on the waiting list was a time of crisis. Little or no relationship exists statistically between such a perception and the number of previous hospitalizations of the potential patient; his or her sex, age, education, or whether the referral agent to the doctor or psychiatrist was the patient himself or a family member.

There is some association between perceiving the situation as critical and being in a self-supporting or essential economic role, as opposed to being in a dependent or merely contributing role. In other words, loss of a breadwinner or self-supporting member, involving a greater financial loss, is apt to be coincident with greater family upheaval in the event of mental illness.

Perception of crisis by family is also related to the duration of the patient's symptoms, though just short of significance. Perception of crisis is more probable either when the patient's symptoms are relatively acute (under six months) or when they have been going on for a long time (five years or more). Between these time periods the family is less likely to feel the situation as critical. The actual symptomatology may also be crucial; and we found a significant relationship between perception of crisis and

the patient's exhibition of overtly aggressive behavior, but no relationship between this factor and whether the patient was depressed or whether he was withdrawn. The identity of the informant made no difference in perception of crisis except that husbands were much more likely *not* to perceive crisis, a finding that further substantiates our earlier one that the conjugal family without children is the more tolerant of the patient's behavior.

Thus this study has indicated that the picture of families of persons who are placed on a hospital waiting list is a complex one, but one which may be studied in terms of the sorts of variables we have examined. Undoubtedly there are scores of other factors which are also crucial and would also be worth study. In any case, the data we have presented here add up to a good deal of knowledge, from which the clinician who finally deals with the patient, whether in the hospital or in a community agency if hospitalization is short circuited, may benefit in assessing the total situation from whence the patient comes and into which, in most instances, he eventually must return, if he is to be rehabilitated.

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Studies on the Sedation Threshold

A. Reproducibility and Effect of Drugs
B. Sedation Threshold in Neurotic and Psychotic Depression

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Introduction

The concept of the sedation threshold was introduced by Shagass [3-9] in 1955, and it has since received considerable attention. The sedation threshold is defined by Shagass as the amount of amobarbital sodium, expressed in milligrams per kilogram of body weight, required to produce an inflection point in the amplitude curve which appears when the frontal 15- to 30-cps electroencephalographic activity is plotted against the amount of amobarbital sodium given. Nearly all such curves contain an inflection point at which the curve, after a steep rise, tends to plateau or fall. The appearance of this inflection point is often coincident with onset of slurred speech, and the latter sign has, therefore, been used as an indication that a sufficient amount of amobarbital has been given.

Shagass found that the sedation threshold was a relatively constant biological characteristic of the normal subject, as indicated by a high test-retest reliability. In neurotics a positive correlation was found between the sedation threshold and the degree of anxiety. A negative correlation was found between the sedation threshold and the degree of ego functioning, in the sense of "contact with reality." Thus, patients with severe anxiety neurosis had the highest thresholds, and patients with organic psychosis had the lowest. Shagass was able with this test to differentiate between patients with neurotic and patients with psychotic depression with remarkable accuracy; the two materials overlapped each other by only 4%. Thresholds were low in psychotic depressions regardless of the degree of agitation or the history of previous depressive episodes.

The purpose of the present study was to examine the sedation thresholds in materials of neurotic and psychotic depressed patients, as diagnosed according to European traditions. Before doing this, we considered it necessary to examine the test-retest reliability of the sedation threshold and to test the effect of chlorpromazine, phenobarbital, and amobarbital on the threshold.

Technique

The sedation threshold was determined as described by Shagass, but with the following modifications.

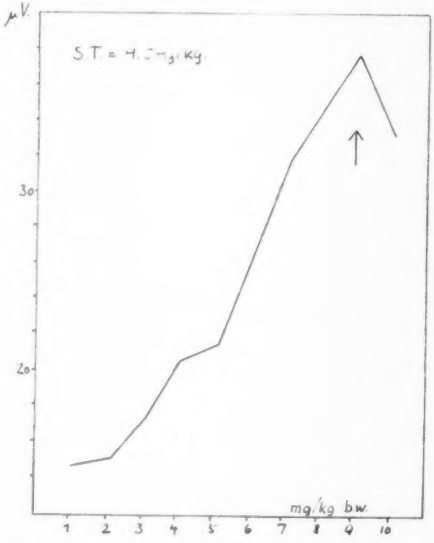

Fig. 1.—Typical amplitude curve. Arrow indicates inflection point.

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1. Amobarbital sodium, which was used by Shagass, produced in our experiments many cases of phlebitis. One patient was tested four times with amobarbital sodium and four times with thiopental sodium under identical conditions. The sedation thresholds, as determined by the inflection point on the EEG amplitude curves, were essentially identical in all eight curves. It was decided, therefore, to use thiopental (Leopental) sodium for the determinations, and since then no complications have been seen.

2. Shagass found that the onset of slurred speech coincided with the inflection point on the EEG amplitude curve and used this clinical test as an indication that a sufficient number of injections had been given. In the present work it soon became evident that this clinical criterion could not be considered reliable. In many cases the onset of slurred speech occurred several minutes before or after the threshold dose as determined on the EEG. In some cases it was not possible to discover slur at all before the patient fell asleep. It was decided, therefore, to rely on the EEG inflection point alone. Injections have been continued until the patient was asleep or until 14 injections had been given.

3. On the EEG only the 15- to 30-cps activity is used for the amplitude curve. Within the 15 seconds preceding the injection the consecutive two seconds of tracing is chosen which contains the maximum fast activity. Within the 10 seconds following the injection, two seconds of tracing is likewise chosen and measured. Measurements are transformed to microvolts, and the two mean amplitudes for each injection period are averaged.

According to Shagass, only the 15- to 30-cps waves occurring in sequences of two or more are measured. We found it difficult, however, to obtain a sufficient number of waves within the two seconds chosen, and therefore even single 15- to 30-cps waves were measured. Two typical curves were measured with and without this modification, and identical inflection points were found.

4. Basing the threshold on the EEG alone makes it necessary to find an inflection point on the amplitude curve. In some amplitude curves, however, several angles may be found where, after a rise, the curve tends to plateau or fall. In Shagass' technical descriptions no solution of this problem has been suggested. After having examined many curves, we have adopted a simple and effective method. The first inflection on the curve with a downward open angle of 140 degrees or less is considered the inflection point, no matter what comes later in the same curve. Practically all curves examined have contained such an angle.

The rule is demonstrated in Figure 2.

All curves were examined by means of an additive ruler. This hand technique is somewhat laborious, but becomes much easier with training.

All patients were selected from the inpatient service of the Risskov Psychiatric Hospital. The sedation threshold was determined 189 times in 100 patients.

Results

Reproducibility of Sedation Threshold.—In order to examine the test-retest reproducibility, nine patients, five men and four women, were examined four times each on

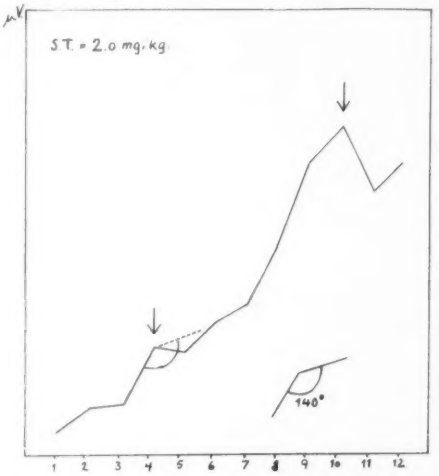

Fig. 2.—Curve demonstrating the 140-degree rule.

TABLE 1.—*Sedation Threshold Examined on Four Consecutive Days*

Patient	Sex	Sedation Threshold, Mg/Kg. B. W. 1st Day	2d Day	3d Day	4th Day
H. R. C.	M	3.0	3.0	3.5	3.5
G. M.	M	2.0	2.5	2.0	2.0
G. A. C.	M	2.0	1.5	2.0	2.0
K. R. W.	M	1.5	2.0	1.5	1.5
G. M.	M	2.5	2.5	2.5	2.0
E. M. P.	F	2.0	2.0	2.0	2.0
E. O. K.	F	2.5	2.0	2.0	2.5
A. W.	F	2.0	2.0	2.0	1.5
E. C.	F	3.5	3.0	3.5	3.0

consecutive days. The patients were 29 to 47 years old. All determinations were carried out at the same time of the day and under exactly similar conditions. If the patients were under drug treatment, the medication was not changed during the four days of examination (Table 1).

Since the barbiturate is not given as a continuous infusion but in separate injections every 40 seconds, it could be expected that the inflection point of the curve would sometimes be nearer one injection and sometimes nearer the following one, resulting in a variation of one injection unit (=0.5 mg/kg. body weight) between the results. To this is added the effect of the individual day-to-day variation. The results obtained did vary only 0.5 mg/kg. body weight, and since this figure is within the experimental error, a day-to-day variation in tolerance could not be demonstrated.

Effect of Chlorpromazine, Phenobarbital, and Amobarbital on the Sedation Threshold.

TABLE 2.—*Effect of Chlorpromazine on the Sedation Threshold*

Patient	Sex	Sedation Threshold, Mg/Kg. B. W. Before Treatment	After Treatment 3d Day	3d Wk.
J. N. J.	M	3.0	3.5	3.0
A. E. B.	M	2.5	2.5	2.0
K. O. C.	M	2.5	3.0	2.5
N. M. S.	M	2.5	3.5	3.5
E. T. P.	M	4.0	4.0	4.5
M. K. B.	F	2.0	1.5	1.5
N. K. M.	F	2.0	1.5	1.5

The sedation threshold was determined in 23 patients before and during treatment with chlorpromazine, and before, during, and after treatment with phenobarbital and amobarbital. None of the patients had received any drug treatment for at least one month prior to the examination, and during the test period none of them received any drug other than the one tested. Of the patients, 16 were schizophrenic, 3 manic-depressive, 3 neurotic, and 1 psychopathic.

Chlorpromazine: Chlorpromazine was given in a dosage of 100 mg. t. i. d., and the sedation threshold was determined three days and three weeks after start of drug treatment. All tests in this and the following material were carried out two and one-half to three hours after the morning dose of the drug. In order to procure as similar external conditions as possible, the investigator and the technical staff were the same in all the experiments in the study.

In six out of seven patients the sedation threshold showed no essential change after treatment with chlorpromazine for three days and three weeks, since a change of 0.5 mg/kg. body weight is considered within the experimental error, as mentioned above. One patient had an increase of 1

TABLE 3.—*Effect of Phenobarbital and Amobarbital on the Sedation Threshold*

Patient	Sex	Sedation Threshold, Mg/Kg. Before Treatment	On Drug 6th Day	After Withdrawal 3d Day	6th Day
		Phenobarbital			
A. C. T.	M	1.5	1.5	2.0	2.5
P. O. P.	M	3.5	2.5	2.5	3.5
E. L. M.	M	2.5	2.0	1.5	1.5
J. L. A.	M	3.5	2.5	3.5	4.5
A. B. N.	M	3.0	3.5	2.0	2.0
O. M. J.	M	1.5	1.5	1.5	1.5
N. P.	F	1.5	1.5	1.5	2.0
		Amobarbital			
N. H. C.	M	2.0	2.5	2.0	1.5
N. S. H.	M	2.5	2.0	2.5	2.0
S. F.	M	3.0	3.5	3.5	4.0
A. P. P.	M	3.0	2.0	2.5	3.0
F. J. S.	M	1.5	1.5	2.0	2.0
H. S. H.	M	3.5	4.0	5.5	5.0
J. C. K.	M	3.0	1.5	2.5	3.0
J. P. W.	M	1.5	1.5	2.0	1.5
P. C.	M	1.5	2.0	2.5	2.0

mg/kg. body weight. None of the patients showed any clinical change during the drug treatment.

Phenobarbital and Amobarbital: Phenobarbital and amobarbital were given in doses of 100 mg. b. i. d. and 200 mg. t. i. d., respectively. The sedation threshold was determined six days after the start of drug treatment. After two weeks of treatment the barbiturate medication was stopped, and the sedation threshold was again determined three and six days after withdrawal of the drug.

Of the seven patients treated with phenobarbital, five showed no change in sedation threshold greater than 0.5 mg/kg. body weight after six days' treatment; two patients had a decrease in threshold of 1 mg/kg. body weight. Three days after withdrawal of the drug, four thresholds did not differ by more than 0.5 mg/kg. body weight from the control value, while on the sixth day only three thresholds were unchanged, the rest of the patients having either a decrease or an increase of the threshold of 1 mg/kg. body weight. There were no clinical changes.

Of the nine patients treated with amobarbital, seven showed no essential change in threshold during and after drug treatment. One patient had an increase of sedation threshold of 2.0 and 1.5 mg/kg. body weight three and six days, respectively, after withdrawal of the drug. There was no clinical change, except that the patient felt quieter and calmer during the treatment.

Sedation Threshold in Neurotic and Psychotic Depression.—The subjects of this examination were 68 psychiatric patients. All patients in this group were classified as neurotic depressive and psychotic depressive. The group of neurotic depressive subjects consisted of patients whose predominant feature was depressive, and a few patients with anxiety as the main symptom. The group of psychotic depressive subjects included patients with manic-depressive psychosis, depressed type, and psychotic depressive reactions. The diagnostic classification employed was made by one psychiatrist for the men and one for the women.

The age and sex distribution of the groups are given in Table 4.

Further analysis of the material shows that the EEG was normal for 35 of the 44 psychotic patients and normal for 20 of the 24 neurotic patients. Of the nine psychotic patients showing a slightly abnormal EEG, two had recently been subjected to electroconvulsive therapy. In the rest of the cases no explanation of the slight abnormalities was found. Of the four neurotic patients showing irregularities of EEG, one was under suspicion for epilepsy; one alcoholic was on disulfiram, and in two cases no explanation of the slight abnormalities was found.

Of the 24 neurotic patients, 1 had received electroconvulsive therapy seven months before the present examination. Another patient had had one electroconvulsive treatment one week before the examination of the sedation threshold. There was no clinical effect of the treatment; his EEG was normal, and his sedation threshold was 4.0 mg/kg. body weight. In the psychotic group 7 patients had been treated with electric shock within one month before the test; 13 of the psychotic depressed had never been treated with electric shock at the time of the present test.

TABLE 4.—*Sex and Age Distribution*

Diagnosis	No. Cases	Sex M	Sex F	Age, Yr. Average	Age, Yr. Range
Psychotic depression	44	18	26	49	29-66
Neurotic depression	24	9	15	37	21-57

TABLE 5.—*Duration of Patient's Illness*

Duration of Illness	Psychotic	Neurotic
Less than a year	13	6
1-2 yr.	6	2
2-5 yr.	7	10
More than 5 yr.	18	6
Total	44	24

TABLE 6.—*Drug Treatment Before and During Test Period*

Drug	Daily Dose, Mg.	Psychotics No. of Cases	Neurotics No. of Cases
Lithium carbonate...	900	1	0
Barbiturates	150	2	2
Chlorpromazine	100-300	12	7
No drug treatment		29	15
Total		44	24

The durations of the patients' illness appear in Table 5.

About half of the patients in the present material received no drug treatment before and during the determination of the sedation threshold. The remaining patients were treated as shown in Table 6.

The distribution of the sedation thresholds in the two groups of neurotic and psychotic depressed patients is shown in Table 7, together with the mean threshold for each group.

A statistical examination of the results obtained shows a difference between the mean sedation thresholds of the two groups which is significant ($t=4.73$; $P<0.001$). Figure 3 is a graphic presentation of the same material after exclusion of two hysterical patients in the neurotic group.

The age of the patients has not been taken into consideration in the present study. Repeated examinations by Shagass have failed to show any correlation between age and sedation threshold.

TABLE 7.—*Sedation Thresholds in Neurotic and Psychotic Depressions*

Sedation Threshold, Mg./Kg. B. W.	Neurotic Depression			Psychotic Depression		
	Men	Women	Total	Men	Women	Total
1.0	0	0	0	1	0	1
1.5	1	0	1	3	8	11
2.0	0	3	3*	5	9	14
2.5	1	3	4	4	5	9
3.0	3	3	6	3	2	5
3.5	2	3	5	1	1	2
4.0	1	0	1	1	0	1
4.5	0	2	2	0	1	1
5.0	0	0	0	0	0	0
5.5	0	1	1	0	0	0
6.0	0	0	0	0	0	0
6.5	0	0	0	0	0	0
7.0	1	0	1	0	0	0
Total	9	15	24	18	26	44
Mean sedation Threshold	3.44	3.17 (3.35)	3.27 (3.39)	2.33	2.17	2.23

* Two of the patients showed hysterical features. When these patients are omitted from the material, the mean values will be as shown in parentheses.

Comment

Some authors have stressed that the technical difficulties involved in the determination of the sedation threshold are considerable (Thorpe and Barker [10]; Boudreau [2]). With the modifications of Shagass' method used in the present study, difficulties have been few and far between.

It appears from the present study that the sedation threshold is highly reproducible. In nine patients tested on four consecutive days the day-to-day variation for each patient did not exceed 0.5 mg/kg. body weight.

It was considered important to examine the effect of various drugs on the sedation threshold. Shagass has not excluded the

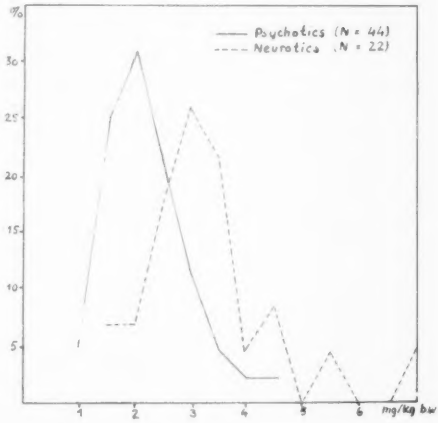

Fig. 3.—Percentage distributions of sedation thresholds, in milligrams per kilogram of body weight, for psychotic and neurotic depression groups. Mean sedation threshold for psychotics, 2.23 mg/kg. body weight; mean sedation threshold for neurotics, 3.39 mg/kg. body weight.

possibility that drug treatment may interfere with the amount of barbiturate necessary to produce an inflection point on the EEG amplitude curve.

Most patients received in psychiatric hospitals have already, for some time, been treated with drugs. This makes it necessary to know whether the determination of the sedation threshold must be preceded by a drug-free period.

In the present study, the effects of chlorpromazine, phenobarbital, and amobarbital have been examined. Table 8 shows that the sedation threshold is but slightly influenced by chlorpromazine treatment in usual therapeutic doses, while amobarbital, and especially phenobarbital, often causes a change in the sedation threshold. The results differ somewhat from those of Bradley and Jeavons,[1] who found significant decreases in the sedation threshold in a number of patients treated with chlorpromazine. The same authors found that reserpine caused no significant change of the sedation threshold.

TABLE 8.—*Change of Sedation Threshold from Control Value During Treatment with Chlorpromazine, Phenobarbital, and Amobarbital*

Drug	Change from Control Value of Sedation Threshold, Mg/Kg. B. W.	Number of Patients on Drug	
		3d Day	3d Wk.
Chlorpromazine	+1.5	0	0
	+1.0	1	1
	+0.5	2	1
	0	2 } 6	2 } 6
	−0.5	2	3
	−1.0	0	0
	−1.5	0	0

Drug	Change from Control Value of Sedation Threshold, Mg/Kg. B. W.	On Drug 6th Day	After Withdrawal 3d Day	After Withdrawal 6th Day
Phenobarbital	+1.0	0	0	2
	+0.5	1	1	1
	0	3 } 5	3 } 4	2 } 3
	−0.5	1	0	0
	−1.0	2	3	2
Amobarbital	+2.0	0	1	0
	+1.5	0	0	1
	+1.0	0	1	1
	+0.5	4	3	2
	0	2 } 7	2 } 7	3 } 7
	−0.5	1	2	2
	−1.0	1	0	0
	−1.5	1	0	0

The conclusion of the present study on the effect of chlorpromazine and barbiturate must be that, although the changes in sedation threshold may not be great, it is advisable to keep the patients free of drug treatment for, e. g., 10 days before determination of the sedation threshold.

Shagass found it possible, by means of the sedation threshold, to distinguish between psychotic and nonpsychotic (neurotic) depression to such a degree that an overlapping of the two materials of only 4% was found. This allows one to draw conclusions which are of importance for the clinical diagnosis in the individual depressed patient.

In our material, a distinction of so high a degree of acuity has not been possible, since a considerable overlapping of the two materials was found, as shown in Table 7 and Figure 3. As for the individual depressed patient, therefore, one can only conclude that a very low sedation threshold will strongly indicate a psychotic depression, without being able to exclude the possibility of a nonpsychotic depression.

On the other hand, a statistical examination of the results obtained shows that the difference between the mean sedation thresholds of the two groups is significant ($t=4.73$; $P<0.001$).

According to Shagass, hysterical patients have as low a sedation threshold as psychotic depressive patients, or even as low as patients with organic brain disease; they seem to form a group of their own. If the two "hysterical" depressive patients are omitted from the neurotic depressive group, the mean sedation threshold of this group is 3.39 mg/kg., as against a mean sedation threshold of 2.23 mg/kg. for the psychotic depressive group. This difference between the two main groups is highly statistically significant ($t=5.10$; $P<0.001$).

Summary

The sedation threshold was examined by means of EEG changes produced by barbiturate. Shagass' method was modified in the following ways:

1. Thiopental sodium was used instead of amobarbital sodium because of complications (phlebitis).

2. The clinical test of slurred speech could not be relied upon and was omitted.

3. Even single 15- to 30-cps waves were measured.

4. As the inflection point of the amplitude curve, the top of the first downward open angle following a rise in the curve was used if the angle was less than 140 degrees.

The test-retest reproducibility of the sedation threshold was examined in nine patients on four consecutive days for each patient. No day-to-day variation could be demonstrated.

The effects of chlorpromazine, phenobarbital, and amobarbital upon the sedation threshold were determined in 23 patients. Only minor alterations in sedation threshold were found, but it was considered advisable to stop drug administration for 10 days before determination of the sedation threshold.

Mean sedation threshold of a group of psychotic depressed patients differed from the mean sedation threshold of a group of nonpsychotic (neurotic) depressed patients, the difference being statistically significant ($P<0.001$). The mean sedation threshold for the psychotic group was 2.23 mg/kg. body weight, as against a mean sedation threshold of 3.39 mg/kg. body weight for the nonpsychotic group.

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Anxiety as a Significant Variable for a Unified Theory of Human Behavior

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I

The maintenance of equilibrium within a healthy range in the presence of environmental changes which are reflected internally is characteristic of all living organisms. Man is no exception in effectively maintaining homeostasis; but, in addition, associated with the evolutionary development of his neopallium, he has the capacity to master his physical environment by changing it to fit his needs and demands.

With his acquisition of consciousness, reflection, and projection into the future, man has also attempted mastery by developing satisfying concepts of his "surround," no matter how small or large it seems to him. These give him a feeling of continuity and certainty and of participating in future effects and causes.

At first they were represented operationally in primitive religious ceremonies, magical rituals, and taboos, which are still part of his transmissible culture. Magic gave way partially to various scientific efforts which depended on the notion that, instead of faith, greater certainty might be developed by observing and predicting changes, and storing information in memory and documents as reservoirs of "truth."

Religions of all types, as well as the sciences and their derived philosophies, represent more or less unified theories of the world and the universe. As various parts of man's world have drawn closer in time, the need for more fruitful and satisfying unified theories has seemed to increase.

Currently the most significant stimulus for unified theories is the rapid acceleration in the acquisition of both empirical and experimental scientific data. The amount of information available to all disciplines of science and the humanities has increased manifold.

Naturalists have disappeared, and philosophers cannot encompass more than a fraction of what comprises contemporary human knowledge. Within the behavioral sciences alone, no one man can learn more than some general principles, a few mathematical abstractions when feasible, and some of the data within a single specialty. Hence, contemporary research and theory building in the behavioral sciences require cooperative efforts within multidisciplinary groups.

Focusing only on the problems of man, several groups of scientists have striven for a unified theory of human behavior, but none has yet even approached success. By attempting to develop abstractions applicable to all phases of human behavior, their goals have exceeded existing logical and mathematical forms, and they have fallen back in part on analogies between observable processes, which is no sin. It at least avoids the dual dangers of reductionism and extensionalism, whereby all behavior is reduced to common bases, such as physicochemical processes, on the one hand, or extends individual psychological properties unaltered to society and culture, on the other hand.

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Attempts at developing common abstractions have been most stimulating, particularly around the conference table, but the cost in omission of respectable data has been too great. Reductionism and its counterpart, on the other hand, must deny the validity of special forms of regularity and specific lawfulness of evolved systems.

It seems to me that it might be possible to achieve compromises by recognizing that there are some processes for which *general commonality* or *systems'* properties may be traced from their biological roots to their social flowering, provided that describable and understandable *specific modifications,* depending on the system under the focus of observation, are also taken into account.

Let us analyze a single function or process as an organizer of behavior recognizable as antecedent to known effects; to less degree, having survival value and, to greater degree, leading to disintegration.

We may view these properties with equanimity as respectable teleology. Then suppose we study this function as it becomes a part of various systems of organizations and observe its relationship to the other parts of a system and to its whole.

Finally, we may study how it is involved in functions of each system in relation to other systems. Each proposition involves separate operational procedures and different frames of reference. If these could be synthesized, a single building block for a unified theory may be at hand.

II

I propose to use anxiety as such a process, which may be observable within the three traditional systems into which we fit human behavior: the biological, the psychological, and the social. I am using biological in a narrow sense, meaning structure, function, and relationships among organ systems.

The psychological system, although certainly closely related to the organic, and especially the central nervous system, is concerned with fantasies, thoughts, language, and feelings. Social systems are concerned with behaviors of individuals within groups of various sizes.

Logically, we should initiate each step of our analysis by considering anxiety as a psychological phenomenon, since it is from this frame of reference, by its definition, that most information has been obtained about its causes and effects.

Indeed, anxiety may be stimulating and facilitative toward goal-directed behavior, problem solving, and learning, or in greater quantities may be a disintegrating, neurosis- and psychosis-developing process. Its consequences, ranging from the progressive to the destructive in a curvilinear manner, are of great importance in man's symbolic behavior and evolution.

Sherrington's famous remark that "the mental is not examinable as a form of energy" has often been quoted. Certainly psychoanalytic theory embodies within its system the 19th century concepts of psychic energy and cathexes or charges, to exclusion.

From what we know today such terms are anachronistic, although their connotations for relationships are still appropriate. It is equally erroneous to apply the principles of individual psychology to the group, as, for example, in stating that "a group thinks" or "a nation is paranoid."

To differentiate the three systems under discussion—the biological, the psychological, and the sociological—and to allow for modifications of a central concept like anxiety by the separate lawfulness of each system, we should use appropriate process terms for each.

Thus, in the biological system we should speak of materials and energy exchange; in the psychological we should speak of input, storage, and output of information, and in the social systems the media of exchange are communications. Since these three modes of operations are distinctly different, they may form the basis of system differentiation as a background for unified theory.

This leads us to an attempt at categorizing anxiety in qualitative terms. *Alertness*

is unconscious and automatic and corresponds to vigilance, a property of all animals. We do not use continual searching movements of the head and eyes, cocking of the ears, or sniffing on the ground; but we do closely watch the eyes and faces of our fellow men. This phenomenon in action is not associated with awareness of physiological concomitants, which remain within the limits of "idling."

The second category is *apprehension,* which is a short-term anticipation of stress occurring before such events as athletic contests, entering new situations, taking an examination, and public performances. We feel keyed up psychologically, along with a dangling vision of possible future failure. We know this feeling consciously and also are aware of the dry mouth, sweaty palms, tachycardia, hyperpnea, frequency of urination, diarrhea, and "butterflies" in the stomach, singly or collectively until action begins. Without these preparations our performances are often less efficient; or, in the vernacular of the stage, we may "lay an egg."

The third category is termed *"free" anxiety,* which may be acute or chronic, lasting for a variable period of time and returning episodically without known external causes, although by the mechanism of projection real events may be utilized as "explanations." Sometimes there may be no precipitating external event; at other times cues in the environment may be followed by free anxiety, but these have secret personal meanings.

When they are universal and public, the response is termed fear. Because stimuli may evoke a long chain of associative thoughts and feelings and only ultimately lead to anxiety, the significant cues are reconstructed with difficulty.

Free anxiety is conscious and reportable (and quantifiable); hence can occur only when the individual has reached a degree of self-differentiation or identity, has a self-reflective consciousness or awareness, and can project himself into the future.

The experience psychologically is unpleasant—in fact, may be associated with an anticipation of impending doom, along with the perception of intense activity of the visceral organs, which feeds back an increased concern and social consequences of "sickness." Because anxiety is so disagreeable, often unendurable, psychological defensive maneuvers are aroused to avoid the unpleasure.

Much of the character and personality deformation, various symptoms comprising psychiatric syndromes, and regressions associated with chronic somatic disturbances are the consequences of prior anxiety.

But, according to our definition, there is no unconscious anxiety, although, when the defensive maneuvers are removed, anxiety may become conscious; or under certain conditions of stimulation or strain, spontaneous in the environment or purposely initiated in experiments, responses may eventuate in free anxiety.

The final category is *panic,* which is characterized by unrealistic behavior, with a wide variety of uncoordinated aggressive, running movements, with consciousness barely maintained in so-called fugue states, violent global visceral accompaniments, and often terminal collapse. Here panic is not reported but is inferred by the behavior, although its initial psychological state may be reconstructed by appropriate means.

The loss of awareness removes the ego's experience of anxiety, reducing the affect as a result of the negative feed-back.

Psychoanalytic theory considers anxiety of two types, derived from the stage of helplessness of the neonate. The first, or *signal* anxiety, functions as a warning and tentative preparation for mobilization of defenses against the breakthrough of overpowering, unacceptable, or conflictful impulses.

The second, or *traumatic* anxiety, is the resultant of failure of signal anxiety to stimulate defenses and/or the overwhelming intensity of the drives that cannot be checked.

From these definitions there arise the following assumptions:

1. Anxiety is an ego or boundary function.

2. Its signal threshold is related to ego strengths and sensitivities which are partly constitutional and partly experimental (learned). A great deal can now be written regarding factors influencing psychological conditioning in early life that prepare the subject for adequate coping with a changing environment. Sufficient feeding, adequate sensory stimulation, etc., contrast with many items indicating inadequate care.

Identification with current culture through incorporation of value systems is necessary for adequate social existence. All of these—proper nutrition, stimulation, ideals, prohibitions, and value systems—seem to be incorporated and effective for preparation against later strains if administered in proper doses at the right times.

3. Behind traumatic anxiety are a series of antecedent learned experiences: somatic, psychological, and cultural.

The functional predisposition or proneness of an individual subject to anxiety reflects his current psychodynamics, the genesis of which is rather complicated.

Persons laden with irrational guilt feelings are more easily stimulated to anticipate harm to themselves—what is psychoanalytically called castration anxiety.

Those who have a great deal of shame are often made anxious by situations that include the possibility of failure. The most effective generalized stimulation for anxiety is isolation, and absence or distortion of communication leading to a feeling of impending disintegration.

In the somatic sphere there are similar general predispositions, such as change of temperature, especially cold, physical trauma, and bleeding. Individual predispositions are not, as in the psychological system, subject to categorization but are referable to early somatic conditioning, which, if known, could form the basis of prediction of intensity and duration of stress responses to a particular stimulus.

In the social field, situations contributing to the feeling of not belonging or not conforming, as evidenced by attitudes of those around, evoke severe psychological reactions, as well as social movements of the individual toward more isolation or toward attacking (schizoid withdrawal—paranoid hostility).

4. Anxiety is cued off in current time by appropriate meaningful stimuli. Thus, proneness and susceptibility to the anxiety response is based on the prior states of the organism. It is patterned from the organism's antecedent psychodynamic experiences, culminating in its patterned structure.

The above statements indicate clearly, I believe, that anxiety is a nonspecific response which may be activated by cues suggesting danger to the person and that it initiates preparatory activities; when these fail, psychological disintegration results. Signal anxiety is a sensitive indicator, an alarm.

Biologically the corresponding process is unconscious and can be recognized by observing the heightened activities of secretory glands (including the endocrines), and of smooth muscles.

What we term the resting, or idling, state is not quiescent. Although relatively silent, it is characterized by well-patterned structural processes of adaptation within a relatively stable internal environment. When this is disturbed, appropriate local peripheral systems become activated, bearing the brunt of adjustment, although with repercussions throughout the organism.

One or more systems may bear the essential brunt of disequilibrium, avoiding a greater spread of homeostatic disturbance. With more intense stimuli or when the stimuli last longer, more and more systems are activated into heightened responses, including central nervous system participation. It is in the later phases that the entire orchestra of visceral activity becomes activated into synchronous, rather than out-of-phase, responses.

Sympathetic and parasympathetic effects in the intact human can only be measured

properly by observing changes of function in organs that these systems innervate, recognizing that the endocrines also interact intimately with peripheral nervous mechanisms.

The great sympathetic outflow from the diencephalon probably is patterned to react more intensely as a result of strains entering the central nervous system by the perceptive systems and acting directly or indirectly via the limbic lobe or visceral brain on lower centers. There is, of course, considerable evidence that a neuroendocrine secretion is mediated by the hypothalamus, acting then on the pituitary body, releasing corticotropin (ACTH).

Corticotropin is thought by some to be liberated humorally by way of the adrenal medulla, which secretes pyrocatechol amines. Others consider that all peripherally and centrally received stimuli influence the pituitary body by activating the central reticular formation.

According to this hypothesis, all stress responses, except for local excitatory reflexes directly under the impact of trauma, are mediated by action of the central nervous system. Whatever structures are associated with the process are thus involved at certain threshold levels.

When the task becomes too great, a signal of alarm is expressed by higher nervous system involvement, including the neuroendocrine nuclei and the hypophysis. Corticotropin is then liberated and the hypophyseal-adrenal axis begins to evidence greater activity.

It is now clear that corticotropin, as well as central vegetative functions, is a sensitive indicator of stress responses and is nonspecific, reacting to a wide variety of stimuli. Thus, corticotropin is somatically of the same order of events as anxiety.

Irreversible damage or diseases known as psychosomatic may follow long-continued stress responses, for which there may be several explanations. Long-continued hypermotility, hypersecretion, and hyperemia may be followed by structural damage.

Toxins involved in the metabolism of stress responders may damage the tissues directly. Finally the stress responses may lead to exhaustion of the participating organs.

Social groups of various sizes are integrated by communications of information via auditory and visual symbols, some verbally spoken and some through mass media.

Through these methods social groups behave largely in conformity with, of course, dissenting independent individualists, usually among the small percentage of intellectuals and, at the other extreme, the psychotic.

The signal of social tension or unrest is a gradual increase in verbal and nonverbal spread of rumor, at first repeated with some accuracy but gradually increasing in distortion, conflict, and speed of dissemination until social disorganization results.

Rumor, too, is nonspecific and signals many kinds of social unrest. As such, it resembles anxiety and corticotropin. Administrators of institutions are well aware that rumors and gripes, outwardly and openly expressed, are signals of interest and vigilance and represent healthy activity.

But beyond a certain point they circulate faster, become more distorted, and are destructive to morale and paralyzing to group effort. Then the administrator steps in with corrective information or modifications of procedures to avoid chaos. Thus, rumor against the background of stable rate of communications is both a signal of impending danger and, in greater extensity, a symptom of its destructive effects.

Theoretical considerations suggest that undifferentiated, multipotentiality for growth is a precursor of differentiated cell groups, organs, or systems.

Differentiation as a process is species- and gene-determined (innate), but requiring specific releasing mechanisms from the environment.

There seems to be a fundamental characteristic of early receptivity for such stimuli, which instigates developmental differentiation. Biologically many perceptive

systems operate, but are basic and fundamental in the service of *incorporation* of material sources of energy. The psychological system receives its content by the *introjection* and storage of memory images of early transactions between child, mother, and still later a variety of others.

These transactions constitute identifications that, when filtered through ego boundaries, indicate the reservoir of appropriate behaviors in various social roles. Still later, the *idealization* of significant elders leads to the acceptance of value systems leading to ethical, moral, and lawful role behavior in social groups.

Excessive stress responses lead to disintegration and dedifferentiation of systems, parts, and their differences from each other, leading to more primitive behaviors in a reversal of the developmental processes of growth and differentiation. Thus we may speak of physiological, psychological, and social *partial* regression to what existed before, inhibited but never lost.

Up to this point we have considered responses in the biological, psychological, and social systems as indicators of stress. The term indicates *responses* to stimuli that are strains.

There is no sharp dividing line between them and the processes that operate to maintain homeostatic equilibrium within the healthy range for the species, the age, or the individual with his special past.

One is quiet, as the healthy usually is—the other disturbing and more or less consciously experienced or observable by gross changes in behavior. Even then the noisy adjustment to severe strains may be reversible and facilitative to learning at any level, to effect an easier or quicker "next time" response.

From earliest life, stress responses continually add information to the repertoire of functional adaptation until the strains reach a variable threshold, forcing the disequilibrium out of the range of adjustment and becoming destructive by creating irreversible conditions—a step jump greater than one.

Generally we can observe that step by step more subsystems become activated whether we view the biological, the psychological, or the social system. One or more processes show evidences of increased activity maintaining healthy ranges of other systems by "bearing the brunt."

But, if strain persists or intensifies, activity spreads until most of all subsystems are in synchronous activity, feeding into one another. Most stress responses within any system have a tendency toward specific latency, sometimes dependent on the build-up of precursor substances.

Thus the timing of the onset of strain is difficult to ascertain. The curve of responses when elastic presents problems for analysis when the initial reaction is rapid and intense, followed by a rebound to below the idling rate and then a slow return to prestress levels.

Thus in all systems the effect outlasts the stimulus, and without longitudinal study it may be thought to be the cause of a new effect (psychological end-phenomenon, sociological-cultural lag).

There seems to be both a species and an individual hierarchical nature to the stress responses. But, finally, the entire orchestra is in synchrony, blasting its noisy adjustments before the stage of exhaustion, regression, or anomie.

When we speak of a hierarchy of responses within subsystems, we naturally must take into account individual variations based on previous experiences or conditioning, which in each system is variously described. For example, intravenous infusions of epinephrine will evoke habitional psychosomatic responses characteristic of the individual's real-life stress reactions.

Statistically we may know the usual mode of timed responses within large groups. For example, a tentative biological order would be corticotropin-17-OH, increase in blood glutathione, increase in amino acids, increased excretion of hippuric acid, etc.

Psychologically, the order of involvement may be first cognitive, then conative, and, finally, affective, changes.

Socially we may conjecture an order of isolation, mass cajoling, appeal to rituals and taboos, and, finally, punitive measures.

Defenses or protective devices against stress responses may be defined from several points of view. If we start with anxiety as a psychological process, there are at least three categories of defenses. In the first the cues may not be recognized by denial of perception or distortion of reality, so that the significance of the stimulus is not perceived—the "I won't look" technique. This is a well-known individual psychological defense, and one utilized by social groups: "The Russians haven't the scientific capacity to develop atomic weapons."

Biologically, elevation of thresholds, usurpation of synapses, and blocking of final common pathways are similar functions. Some psychiatrists mutilate the frontal lobes or lower nervous excitability in the central reticular formation by drugs to facilitate such defenses.

The second group of psychological defenses is directed toward the affect of anxiety itself, which is difficult for man to endure.

It maintains alertness and interferes with sleep and is usually physical and mental "unpleasure." Man ascribes it to external causes, calls it fear and moves away from the projected situation or person. Or he sets into operation numerous psychological defenses which become, as rituals, phobias, withdrawals, and regressions, the symptoms of psychiatric syndromes. Social unrest is also dealt with by devices of repressing, isolating, and denial.

Biologically the stress indicators themselves tend to decrease the source of their stimulation; for example, excessive 17-OH decreases the amount or rate of production of corticotropin, which is the precursor to 17-OH (negative feed-back).

In all of these second-order defenses, once thresholds have been attained, we may speak of feed-back. The defensive maneuvers, the social restrictions, the endocrine secretions, which are liberated with the recognition of danger after a latent period of denial, influence the source of their stimulation by decreasing their intensity, thereby initiating a benign circle, instead of an ever-increasing stress response.

In the case of corticotropin and 17-OH, the feed-back is negative. Anxiety stirs up new ego defenses against its strength, and added repressive maneuvers result. Social unrest tends to be followed by tighter controls. The Hungarian revolution was followed by Russian intensification of punitive restrictions. These are positive feed-backs, but their effects are similar.

The third category of defenses is directed against the effects of anxiety. Ego functions become strained to decrease the difficulties arising from lengthened perception-decision time, inaccuracy, decrease in confidence, and inability to learn.

The source of danger becomes generalized, objectless; and discriminatory functions are lost. There is usually an accompanying feeling of disintegration, and this last-ditch defense may actually be a psychotic regressive break.

Biologically, heightened stress responses require the detoxification of metabolic and endocrine products, straining such organs as liver, spleen, and kidneys. Socially, decrease in confidence, irrational group behavior, and loss of group values may end in heroic defenses, escape of minorities, or revolution against governmental leaders.

At this point the questions of purpose or teleology arise. For anxiety to be viewed as facilitative, stimulating to learning and maturation, seems reasonable, but not its destructive effects when external dangers are absent. Furthermore, graded responses to external dangers could serve adequately as learning experiences. Why do we react as though we were afraid of our inner impulses by responses similar to fight or flight when there is no outer object to attack or from which to flee?

Lorenz states this well:

> The biological meaning of a reaction and the subjective purpose given to the animal have nothing in common and must on no account be identified. The extreme type of reaction that is senseless owing to lack of external conditions is the *reaction in vacuo* which is discharged without an object.

It's a kind of psychological stupidity (from which we have not yet evolved) to get prepared over and over again for events which are expected but never occur and which we know intellectually. Outstanding leaders know this well and keep attuned to the ever-possible danger of destructive rumors, which have overthrown even the strongest despots with fully armed cohorts.

Finally, biologically the signal corticotropin in the absence of physical stimuli and its stress responsiveness in liberating 17-OH have no known utilization as yet. Secreted, excreted, detoxified it is, but we still do not know what utility results from its presence or from its derivatives in dealing with the many physical and psychological stimuli to which it responds.

III

In the preceding brief overview, I have discussed the biological, psychological, and social systems as isolated foci, as, in truth, is necessary for an analysis of their functional processes and part-whole relationships. In life they are constantly in transaction by means of cyclic reverberating communications, silent or noisy, depending on the amount of strain and stress responses.

Like the parts of each system that are activated often to extreme, thereby stabilizing the remainder of the system, so one of our three systems may handle the strain without much or any disturbance within the others. As the strain increases, the stress responses tend to spread.

When stress is activated within a person, it may be followed by unpleasurable anxiety or biological disequilibrium. Each of these systems seems susceptible to respond first, depending on the quality and quantity of the stimulus and the state of the organism.

Some of our paratrooper subjects reacted with biochemical alterations first, before experiencing anxiety, and vice versa. Disturbed behavior, such as awkward coordination or alterations in social contacts, may also appear first, associated with or followed by anxiety and/or biological responses.

The biologically disturbed person may become sick and disrupt his family life; the anxious person may be the focus of social disorganization. Social unrest is associated with increase in personal anxiety and somatic complaints. Paranoid defenses against anxiety are often followed by loss of social communication and isolation, augmenting anxiety.

Many other examples may be cited. In general, the intersystem transactions when homeostasis is mildly disturbed lead to obvious reactions in single systems and probably cooperative defenses in others. When the disturbance increases in intensity, other systems are activated, first to initiate more vigorous defenses and finally in a regressive breakdown.

Somatopsychic-social dedifferentiation may be epitomized by persons showing intense global visceral discharges, panic, and destructive, objectless social behavior.

It is only the intense transactional effects that are obvious. We still do not have the tools to measure subtle psychosocial changes, nor are we sure of their significant variables. However, we should keep always in mind the transactional nature of homeostatic or feed-back controls among systems and their part-whole relations. Such is the essence of transactionalism.

This point of view requires two fundamental operational principles, which are difficult to implement. The first is simultaneity of observation of change through time, taking into consideration the need to include single, multiple, and continuous measurements, often necessary for the study of parts of the intact human.

The second principle is some kind of uniform system of casting measurements of all variables into numerical symbols for the use

of computer systems. For the present, multivariate analysis is, unfortunately, deficient and very expensive!

IV

Reiser, of the University of Pittsburgh, chides our contemporary scientists for not integrating the body of knowledge created by science into a unified interpretation of man, his place in nature, and his potentialities for creating the good society. He states that man requires a world view and a theory of the universe he inhabits, which up to now has been attempted only by formal religion and philosophy.

Religion, or the supernatural, of course, requires no contact with the observable world, whereas philosophy requires the data of nature. Metapsychology in the Freudian sense represents a structural way of viewing human behavior through its mental phenomena, dividing it into topological, economic, and dynamic facets.

It is the closest approach to philosophical reconciliation of the observable and measurable phenomena of man's nature and represents an attempt to give a broad sweep of meaning to psychological man. However, philosophy, which reiterates, refines the expression, or modifies the semantics of science, adds little to meaning or to teleology. Neither does understanding of the human mind in health or illness as a process of *isolated* psychodynamic research add much to universal meanings.

As a matter of fact, the research for a unified theory which has meaningfulness is fragmented among the various disciplines in which relationships are studied. From protoplasmic irritability to social activity, many disciplines are concerned with viewing man in his relationship to nature. A unified concept in the behavioral sciences must include at least biology, psychology, and sociology.

In this brief essay, I have used anxiety as an example of how unified theory of human behavior might be constructed on several major concepts significant for all of behavior, biological, psychological, social, etc., modified by the knowns of the systems among which unification is possible. Certainly, such unified theory might not create a synthesis of wisdom or a new world philosophy, as Reiser indicates, but it will help to stimulate further investigation.

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Urinary Excretion of Phenolic Acids by Normal and Schizophrenic Male Patients

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It has been reported that schizophrenics exhibit a general "aromaturia."[1-4] The psychotomimetic effects of numerous aromatic organic compounds suggest that schizophrenia may be caused by endogenous production of a psychotogenic metabolite of a naturally occurring aromatic compound, such as phenylalanine, tyrosine, or tryptophan. Confirmation of the finding that the urine of schizophrenic patients contains unusual levels or types of aromatic compounds would favor the hypothesis that schizophrenia is caused by derangement of the metabolism of aromatic compounds, which, in turn, leads to the endogenous production of a psychotogenic agent.

The recognition and control of numerous irrelevant variables represent a recurrent difficulty in the study of schizophrenia. As part of a broad research program on the biological aspects of schizophrenia being carried out in this laboratory,[5] both normal volunteers and a carefully selected[10] group of schizophrenic patients have been maintained in similar environments and on essentially the same diet for prolonged periods of time. This presented an opportunity to make objective and quantitative determinations of urinary phenolic acid excretion by subjects from each of these groups. The results of these quantitative measurements are reported in this paper.

Methods

The urinary excretion of certain phenolic acids has been shown repeatedly to be markedly influenced by diet.[6-9] For this reason, the urinary excretion of these compounds was studied during a period in which the patients had been placed under careful dietary control by another group in our laboratory.[10] The study was divided into three phases: 1. A normal diet was offered to both groups, but no attempt was made to control additional food intake by the normal subjects. 2. The normal diet was continued with the addition of approximately 1 gm. of ascorbic acid per day. 3. For a six-week period a low ascorbate diet (about 5 mg. per day) was supplied; milk and green vegetables were omitted, and eating outside the hospital by the controls was forbidden.

Seven male schizophrenic patients, with an average age of 33 years, all of whom had been hospitalized for at least five years,[11] and six normal male subjects, aged 18 to 21, who had volunteered on the basis of religious convictions, constituted the experimental sample. These two groups were maintained on separate wards but received the same diet.

In order to make valid intergroup and interindividual comparisons, the rate of excretion of phenolic acids rather than the urinary concentration of these materials was estimated. Although creatinine values have frequently been used to calculate the hourly urinary excretion, there is some doubt as to the consistency of creatinine excretion even in normal subjects[12]; in addition, schizophrenics generally show more variability than normal populations in many biological measures. By vigilance on the part of the nursing staff, it was possible to obtain complete 24-hour urine collections from each subject. Six 24-hour urine specimens, two from each subject during each of the three phases of the study, were analyzed.

An aliquot from each urine, equivalent to the volume excreted over a 15-minute period, was diluted to 50 ml., acidified with hydrochloric acid, and saturated with sodium chloride. This solution was extracted with 60 ml. of water-saturated ethyl acetate. A 50 ml. aliquot of the ethyl acetate extract was evaporated to dryness in a beaker with a stream of cold air. The residue was transferred to a 4 ml. vial with three 1 ml. portions of ethyl acetate-50% ethanol, and the resulting solution was again evaporated to dryness under a stream of cold air. The residue in each of the vials was dissolved

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in 1 ml. of the ethyl acetate-50% ethanol mixture. Twenty microliters of the extract, corresponding to 0.5 minute's urinary excretion, was applied in 5μl. portions to 9×9 sheets of Whatman No. 1 filter paper for two-dimensional chromatography. Quadruplicate paper chromatograms were developed two-dimensionally in isopropanol-ammonia-water (8:1:1 by volume) and in benzene-propionic acid-water (2:2:1 by volume).[13] One set of duplicate chromatograms was sprayed with diazotized sulfanilic acid (DSA), and the other set with diazotized *p*-nitroaniline (DPNA).* The optical density of each spot was measured on a slightly modified Photovolt Model 52 densitometer, using Wratten filters and an aperture of 6 mm. diameter.

* DSA and DPNA were prepared according to the method described by Dr. M. D. Armstrong (personal communication, 1958). Sulfanilic acid 1 gm. was dissolved in 500 ml. of 2% HCl; 1 gm. of sodium nitrite was dissolved in 500 ml. of distilled water. The two solutions were cooled to 5 C, and equal volumes were mixed; to this solution of diazotized sulfanilic acid was added an equal volume of cold 10% sodium carbonate. The solution was sprayed immediately. The DPNA reagent was prepared in an analogous manner, using 0.5-gm. *p*-nitroaniline in 500 ml. of 2% HCl, in place of the sulfanilic acid.

The range of recoveries of standards added to urine was 55%-85% for different phenolic acids, but had a smaller range in the case of any single compound.

Results

The paper chromatograms showed 13 different spots,[13] which were measured densitometrically. Most compounds were detected by both DSA and DPNA; 3-methoxy-4-hydroxyphenylhydracrylic acid (Spot 18)[13] was detected only with DPNA, and feruloylglycine (Spot 16)[13] was detected only with DSA. In addition, several other substances appeared only occasionally in the serial urine specimens of certain subjects.

For each of the spots, optical-density measurements were obtained, and the values from the duplicate chromatograms were averaged. No significant intraindividual variance was found between days within an experimental phase or between experimental phases.

For this reason, the optical densities of each spot, after spraying with DSA or DPNA, were averaged for each subject over

TABLE 1.—*Excretion of Phenolic Acids by Normal and by Schizophrenic Males*

	Optical Density of Spots Observed in Urine Extracts *												
Subject	1	2	3	6	7	8	10	11	12	13	16	18	21
						Schizophrenic Patients							
1	0.31	0.80	0.62	0.12	0.11	0.06	0.33	0.36	0.20	0.12	0.015	0.13	0.12
2	0.32	0.92	0.63	0.14	0.05	0.38	0.13	0.31	0.13	0.12	0.044	0.02	0.06
3	0.32	1.15	0.94	0.03	0.09	0.24	0.34	0.20	0.12	0.08	0.034	0.06	0.43
4	0.33	1.08	0.84	0.05	0.08	0.11	0.42	0.26	0.14	0.08	0.055	0.025	0.20
5	0.18	0.89	0.67	0.00	0.01	0.11	0.08	0.10	0.09	0.11	0.004	0.02	0.08
6	0.30	0.78	0.65	0.00	0.08	0.03	0.29	0.23	0.12	0.10	0.021	0.08	0.03
7	0.25	0.51	0.44	0.00	0.06	0.09	0.18	0.26	0.09	0.14	0.00	0.00	0.00
						Normal Subjects							
A	0.32	0.34	0.40	0.27	0.08	0.00	0.22	0.33	0.17	0.04	0.00	0.00	0.00
B	0.25	0.40	0.34	0.04	0.05	0.05	0.16	0.23	0.09	0.09	0.00	0.03	0.01
C	0.26	0.51	0.59	0.14	0.02	0.02	0.29	0.21	0.15	0.08	0.00	0.01	0.00
D	0.35	0.30	0.37	0.23	0.05	0.08	0.18	0.23	0.09	0.10	0.00	0.04	0.00
E	0.23	0.30	0.42	0.13	0.04	0.00	0.26	0.13	0.09	0.17	0.00	0.00	0.01
F	0.36	1.12	0.92	0.16	0.16	0.18	0.47	0.32	0.17	0.03	0.106	0.45	0.14
U	18	6	7	5	16	8	15	20	21	12.5	6	15	5.5
P	0.730	0.034	0.052	0.022	0.534	0.074	0.446	0.546	1.05	0.132	0.034	0.446	0.028

* Spots were measured with densitometer after DSA spray except for Spot 18, which was measured after DPNA spray. The spot numbers refer to the following compounds: 1, *p*-hydroxyphenylacetic acid; 2, *m*-hydroxyphenylhydracrylic acid; 3, *m*-hydroxyhippuric acid; 6, salicyluric acid; 7, homovanillic acid; 8, dark red DSA, gray DPNA; 10, 3-methoxy-4-hydroxymandelic acid; 11, *m*-hydroxyphenylacetic acid; 12, *p*-hydroxybenzoic acid; 13, *o*-hydroxyphenylacetic acid; 16, feruloylglycine; 18, 3-methoxy-4-hydroxyphenylhydracrylic acid; 21, *m*-hydroxyphenylpropionic acid. (The numbers of the spots refer to the chromatographic map in Armstrong's report.[14])

the six separate days of urine collection (Table 1). The Mann-Whitney test [14] was applied to the average optical-density values for comparison of the results of the two groups, since this test requires only ordinal data and can be used with small samples. The value of U for each column in Table 1 is a measure of the overlap of values from the two populations; the P value has its usual meaning.

The schizophrenic group excreted significantly higher levels of four phenolic acids. The numbers are those used by Armstrong et al.[13]: *m*-hydroxyphenylhydracrylic acid (Spot 2); *m*-hydroxyhippuric acid (Spot 3); feruloylglycine (Spot 16) and *m*-hydroxyphenylpropionic acid (Spot 21). The first two acids were identified by cochromatography with authentic compounds † and by color reactions [13,15]; the latter two were identified tentatively by R_f values and color reactions.

Closer inspection of the data for these four compounds showed that six of the seven schizophrenics accounted for the high excretion of that group, whereas only one of the normal controls consistently excreted comparable amounts. Since caffeic and chlorogenic acids [8] and coffee [9] have been reported to give rise to increased excretion of these phenolic acids, the coffee-drinking habits of the subjects were investigated (Table 2). It was found that the six schizophrenic patients and the one normal control who showed the high excretion rates of these compounds were the only ones of both groups who drank coffee. This analysis shows, furthermore, that two additional phenolic acids were elevated in the urine of coffee drinkers: 3-methoxy-4-hydroxyphenylhydracrylic acid and Armstrong's Spot 8 (unidentified).

All six of these phenolic acids have been reported as metabolites of coffee and of chlorogenic acid, although Booth et al.[8] did not find *m*-hydroxyphenylhydracrylic acid and Shaw and Trevarthen [9] did not note an increase in *m*-hydroxyphenylpropionic acid after a massive dose of coffee. Some of the slight discrepancies between our study and these two may be explained by the chronic ingestion of coffee by our subjects.

† The *m*-hydroxyphenylhydracrylic acid was generously supplied by Dr. Charles C. Sweeley; the *m*-hydroxyhippuric acid was isolated from urine after ingestion of 5 gm. of *m*-hydroxybenzoic acid.

TABLE 2.—*Relationship Between Coffee Drinking and Phenolic Acid Excretion* *

Subject	Spots Observed in Urine						Coffee Drinking
	2	3	8	16	18	21	
			Schizophrenic Patients				
1	0.80	0.62	0.06	0.015	0.13	0.12	+
2	0.92	0.63	0.38	0.044	0.02	0.06	+
3	1.15	0.94	0.24	0.034	0.06	0.43	+
4	1.08	0.84	0.11	0.055	0.25	0.20	+
5	0.89	0.67	0.11	0.004	0.02	0.08	+
6	0.78	0.65	0.03	0.021	0.08	0.03	+
7	0.51	0.44	0.09	0.000	0.00	0.00	−
			Normal Subjects				
A	0.32	0.40	0.00	0.000	0.00	0.00	−
B	0.40	0.34	0.05	0.000	0.03	0.01	−
C	0.51	0.59	0.02	0.000	0.01	0.00	−
D	0.30	0.37	0.08	0.000	0.04	0.00	−
E	0.30	0.42	0.00	0.000	0.00	0.01	−
F	1.12	0.92	0.18	0.106	0.45	0.14	+
U_{coffee}	0	0	5	0	4	0	
P_{coffee}	0.002	0.002	0.022	0.002	0.014	0.002	

* All conditions as in Table 1, except that U- and P-values have been calculated on the basis of coffee drinking or non-coffee-drinking.

Salicyluric acid (Spot 6)[13] was elevated in some of the individual urine specimens from both groups. Although acetylsalicylic acid U.S.P. (aspirin) was restricted on both wards, the two schizophrenic patients with significant amounts received medication which contained salicylates, and the normal subjects may have ingested salicylates during their time outside the hospital. The erratic day-to-day fluctuation of the excretion of salicyluric acid, and its known relation to the use of acetylsalicylic acid (aspirin) and other easily obtainable salicylates, make studies of this metabolite of little value as an indicator of disturbed or abnormal metabolism.

With the exception of the six coffee-derived metabolites and of salicyluric acid, no other significant differences were found between the urinary excretion of other phenolic acids by the two groups.

Comment

In a study to investigate the occurrence of an abnormal metabolism of aromatic compounds in schizophrenic patients as a possible cause of this disease, the paramount requirement is to demonstrate unequivocally either the presence of abnormal metabolites or a statistically significant difference between normal subjects and schizophrenic patients. Metabolites of many types of compounds are excreted in the urine; urine is relatively easy to obtain, and it is available in relatively large quantities. These facts make this biological fluid worthy of study in metabolic investigations. However, urinary constituents reflect not only metabolic processes but also dietary intake. The dietary influence on the urinary excretion of certain phenolic acids has been repeatedly pointed out in the reports by Armstrong et al.[6,7,13] This study of the urinary excretion of phenolic acids was initiated on these subjects because it was believed that they were under very strict dietary control. Both groups were hospitalized; the same food was delivered to each ward, and an effort was made to prevent eating fads on the part of the schizophrenic patients. With the subjects under this strict supervision, the finding that the urinary excretion of four of the phenolic acids was significantly higher in the schizophrenic group appeared very interesting and seemed to indicate that there was an abnormal metabolism of phenolic acids in schizophrenic patients. If the investigation had stopped at this point, we would have arrived at an erroneous conclusion. However, consideration of the possibility that this difference in the urinary excretion of these phenolic acids may have been due to an uncontrolled dietary factor, suggested by the report of Booth et al.,[8] led to the finding that coffee drinking was more closely related to the difference in urinary excretion of the particular phenolic acids than was the presence of schizophrenia.

This finding emphasizes the importance of a careful search for uncontrolled variables when metabolic differences are found between schizophrenic patients and normal controls, and the high sensitivity of paper chromatographic methods to certain dietary variables.

In studies of metabolism where intersubject and intergroup comparisons are to be made, it is necessary to correct for differences in concentration of the urine specimens when preparing the paper chromatograms.

In this study a volume of each urine specimen representing the excretion of a 15-minute period for that specimen was selected for extraction. These portions of the urine were diluted to a constant volume (50 ml.), were saturated with salt, and were extracted with ethyl acetate. This procedure had the advantage of providing us with a concentrated extract of the urine specimens suitable for paper chromatography; the salt saturation and the dilution to a constant volume made the extractions reproducible.

Although an elevated excretion of phenolic acids has been reported in scorbutic guinea pigs, it is not surprising that no effect was noted in our studies on

human subjects, since no detectable clinical symptoms of scurvy were found even after six weeks of an ascorbic-acid-deficient diet.

The results reported here do not confirm those reported by McGeer et al.[1,2] but are in agreement with those of Acheson et al.[16] The extensive investigations of Armstrong et al.[6,7,13,17] have not revealed any consistent differences in the diazo-reacting[13] or Ehrlich-reacting compounds[17] in urine between patients with various forms of mental disease and normal subjects.

Therefore, at this time there is no unequivocal evidence that a difference exists between the metabolism of aromatic compounds by schizophrenic patients and that by normal subjects.

Summary

The urinary excretion of phenolic acids by male schizophrenic patients and normal male subjects while on a supposedly uniform diet has been investigated. A statistically significant difference between the two groups was found for four phenolic acids. Since these same compounds had been reported as metabolic products of substances present in coffee, a careful examination of the coffee-drinking habits of the subjects under study was made. The excretion of these four compounds and two others, in addition, was found to correlate at a highly significant level with coffee drinking.

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A Comparison of Group-Centered and Individual-Centered Activity Programs

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A recent study by Levine, Marks, and Hall [1] compared the effects of different physical medicine and rehabilitation (PMR) activities and of different therapists on the behavior and symptoms of mental patients. In their comparison of the effects of a traditional occupational therapy (O. T.) program and a lawn-mowing detail, it was found that the psychiatrist, without being aware of whether the patient had gone to O. T. or to lawn mowing, rated the patients assigned to lawn mowing as being readier for discharge from the hospital; as having become less withdrawn, less depressed, less suspicious; as having a greater diminution in disorders of thought, and as having shown greater improvement in hospital adjustment. In addition to the greater improvement of the lawn-mowing group, it was found that the personality of the two therapists involved affected the benefit of each activity.

Three possible explanations for the better showing of the lawn mowers were offered.

1. "More group feeling and more patient interaction existed in the lawn-mowing details." 2. "It is possible that the patients interpreted lawn mowing as a necessary job in terms of hospital maintenance, while O. T. was seen more as 'play therapy.' If this is true, it may follow that greater ego gratification ensued from a real job well done rather than from 'busy work.'" 3. "The projects undertaken by the patients (in O. T.) were difficult and complex; there was, consequently, more chance for errors in judgment. These mistakes would often lead to frustrating experiences and place the patient under great stress."

The study being reported upon was undertaken to test the first two hypotheses, namely, that group feeling and the ego gratification derived from a real job were salient factors leading to improvement. The activities chosen were (1) traditional O. T. and (2) a group-centered woodworking project in which the patients made lawn chairs for use on the hospital grounds. The group activity was designed so that there would be a maximum amount of patient interaction, in that most processes required two or more people working together. As a side-light, we were interested in whether there would be any differences between two therapists who had markedly different personality structures but who were both felt to be good therapists by their supervisors.

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This study was conducted in connection with the Veterans Administration's Psychiatric Evaluation Project, directed by Richard L. Jenkins, M.D. This paper was reviewed in the Veterans Administration and is published with the approval of the Chief Medical Director. The statements and conclusions of the authors are the result of their own study and do not necessarily reflect the opinion or policy of the Veterans Administration.

The authors wish to express their gratitude to Dr. A. E. Trollinger, Manager; to Miss Adele Matelson, R.N., and Mr. Zigie Zallanski and their staff of the ward involved; to Dr. Leon Cohen, who administered and interpreted the psychological tests, and to Mr. Art Fagen and Mr. George Mullan, the two industrial therapy aides who conducted the details and rated the patients. Especial thanks are due to the two activity therapists who conducted the groups but whose names are withheld because of the personal nature of the test findings.

Procedure

Material.—From a building of about 160 patients on the acute intensive treatment service the ward psychiatrist (H. K. M.) selected all those nonlobotomized schizophrenic patients who were likely to remain hospitalized for more than two months. From this pool of patients four groups of 20 patients each, equated for age and duration of current hospitalization, were selected. The mean age of the sample was 36, the ages ranging from 25 to 50 years. Of the 80 patients, 15 had been in the hospital for less than six months, 28 from six months to two years, and 37 for more than two years. Length of current hospitalization was used for stratification, since it was felt that for short-term prognosis this was a more appropriate single variable than total length of previous psychiatric hospitalization. There were two PMR therapists, and the four groups were randomly assigned, so that each therapist had one occupational therapy group and one woodworking group. These activities were scheduled for an entire morning or an entire afternoon. In order to minimize, insofar as possible, the effects of other activities the patients might be involved in, they were assigned for the remaining half-day to an outdoor industrial therapy detail. The industrial therapy activity consisted of such jobs as grading, shoveling snow, and installing drain tile in fields. The industrial therapy groups were so constituted that each consisted of 10 patients who were in O. T. and 10 who were making lawn chairs. The study extended over a 12-week period.

Method of Evaluating Change in Patients.—Four independent ratings were used to determine whether there were any changes in the patients following the experimental procedure.

1. The ward psychiatrist rated each patient, without awareness of which group he was in, as to whether there had been any change on 10 variables primarily related to psychiatric symptoms and hospital adjustment.*

2. The PMR therapists rated each patient at the beginning and at the end of the experimental period on a 14-item scale,† dealing with such variables as assertiveness, attempts to be helpful, consistency and effectiveness of work, nature and extent of interaction with the therapist and with other patients, and appearance.

3. The industrial therapy aides also completed pre- and postratings on the same 14-item scale.

4. Most patients were evaluated at the beginning and at the end of the experimental period by the Wittenborn Standard Interview. This device is currently being standardized by Dr. Richard Wittenborn, of Rutgers University. Briefly, it consists of an interview wherein, among other things, the subject is asked 15 questions, such as these: "Tell me about the kind of work you'd like to be doing five years from now." "How long have you been planning to do this kind of work?" "What kind of people are hard for you to work with?" or "What would people who don't like you say about your personality?" Latency of response, duration and pattern of response, and associated body movements are recorded.

Results

Of the original 80 patients, 74 completed the experimental period, 38 remaining in the O. T. group and 36 in the woodworking group.

Activities Compared.—A mean improvement score was derived from the psychiatrist's ratings by using the following scaling system for each of the 10 items: −1, worse; 0, unimproved; 1, improved; 2, much improved.

The mean score was arrived at by averaging all items that were applicable for each patient. The Table gives a summary of the psychiatrist's ratings. The obtained results do not support the hypothesis that one of the activities is more beneficial than the other (analysis of variance). When each item of the scale was analyzed independently (χ^2), there were no significant differences among any of the 10 symptoms. Similarly,

Mean Improvement Scores

Therapist	O. T.	Lawn Chairs	Combined
A	0.28	0.29	0.28
B	0.30	0.32	0.31
Combined	0.29	0.30	0.30

* These items are part of a scale developed by the Psychiatric Evaluation Project, Dr. Richard L. Jenkins, Director. They consisted of rating the patient as much improved, improved, unimproved, or worse on each of the following variables, when applicable: (1) withdrawal (contact with reality), (2) disorder of thought, (3) paranoid suspicion, (4) excitement, (5) depression, (6) unrestrained impulsiveness or other evidence of lack of consideration of future, (7) excessive hostility, (8) psychosomatic, hysterical and/or hypochondriacal complaints, (9) hospital adjustment, and (10) over-all psychiatric condition.

† Copies of this scale can be obtained by writing the authors.

analysis of the mean change in the ratings of the PMR therapists and industrial therapy aides showed no significant differences between groups. When each subscale was considered separately, 3 of the 14 items showed differences significant at the 5% level. The lawn-chair group showed a greater increase in the duration of their responses to the therapist, while the O. T. group showed a greater increase in the amount of time they spent interacting with the therapist. The third significant difference was in the item which required the therapist to rate his feeling toward the patient. The choices ranged from *1* ("I would prefer not having this person in my group") to *5* ("I would like to have him as a friend"). There was greater positive change in feeling for patients in O. T. than for those in woodworking. None of these three differences between groups were noted in the patients when they went to the industrial therapy detail. All 14 comparisons with the industrial therapy aides' ratings were nonsignificant.

There were no differences between the changes in groups on either the latency or the duration of response on the Wittenborn Standard Interview.

Therapists Compared. — Psychological testing revealed that Therapist A is of superior intelligence, can use his intelligence more effectively, possesses more energy which can be directed outwardly, is more interested in people, and can form warmer interpersonal relationships. . . . Therapist B is a much less organized and effective person than A despite his attempts at compulsive defenses and pedantry. He appears to be the less energetic, by far the less spontaneous, and by far much more withdrawn than A. Seeing the pathology of the patients is quite likely to threaten his own, rather tenuous, security.

The Table contains a comparison of Therapist A's patients and Therapist B's patients on the psychiatrist's ratings. There were no significant differences between therapists. Analysis of the 10 individual items of the scale also yielded no differences between therapists. Similarly, there were no significant differences on the mean improvement score derived from the industrial therapy aide's ratings. However, Therapist B's patients were rated as having shown greater improvement in both the consistency and the effectiveness of their work. These differences were significant at the 1% level. At the 5% level there was an increase in positive feeling toward the patients of Therapist B.

A significantly greater number of Therapist B's patients showed an increase in the duration of their responses on the Wittenborn Standard Interview. Although only approaching significance (P between 0.10 and 0.05), Therapist B's patients showed a greater increase in latency of responses. When you consider that Therapist B is a slower, more deliberate person, it seems as though his patients tend to acquire his typical mode of responding.

Comment

The study being reported on indicates that neither group activity in and of itself nor ego gratification derived from a real job was sufficient to produce changes of the same magnitude as those resulting from lawn mowing. How, then, may we account for the therapeutic effectiveness of lawn mowing? At present, the most compelling aspect of the previous experiment which was not duplicated is the inherent simplicity of mowing. The patient is not called upon to make relatively complex judgments, as is required in many O. T. projects and in the process of building lawn chairs. He need not undergo the stress engendered by fear of failure. He has no choice. He does not decide what project he would like to do. Perhaps it is this freedom from stress and decisions which allows for improvement.

Another aspect worth mentioning is the group affective reaction aroused by the lawn mowing. Many of the patients bitterly resented and complained of having to mow lawns, particularly when on occasion they saw a tractor with a gang of seven mowers

speedily and effortlessly doing the same job they were assigned. It may be that each, serving as a catalyst to help the other express his resentment, obtained some measure of catharsis, and also of feeling closer to others, thereby creating a wedge in their curtain of withdrawal.

For some patients the unpleasantness of the task may have created an impetus for improvement, or at least for movement. This was clearly seen in one patient who left the hospital, against medical advice, after one week on the experimental program because he resented having to work every day. It may also have been a factor leading to the running away of two patients during the first week. Apparently, removing some of the secondary gains from the role of patient can allow for healthier motivation to manifest itself.

Our study adds some support to the notion that with many activities the personality of the therapist is more important than the activity itself.

Summary

The primary purpose of this study was to compare the effectiveness of an individual-centered activity (O. T.) and a group-centered activity (making lawn chairs for use on the hospital grounds) in the treatment of schizophrenic patients. As a result of a previous study,[1] it was felt that group feeling and the ego gratification derived from a real job were salient factors leading to improvement.

Eighty nonlobotomized schizophrenic patients were divided into four groups, matched for age and length of current hospitalization. Each group was assigned to one of the experimental activities for half a day and to an industrial therapy detail for the other half-day. At the onset of the study the patients were rated by the activity therapist, the industrial therapy aides, and a psychologist. After 12 weeks they were reevaluated by the same procedures. In addition, the ward psychiatrist rated the changes shown by each patient over the course of the 12-week period.

The results indicate that, essentially, there were no significant differences between the effectiveness of the individual-centered and the group-centered activity. However, there is some support for the hypothesis that, for the activities studied, the personality of the therapist is more important than the activity per se.

Further research is needed to determine the effects of the complexity of an activity on patient improvement, and also to study the effectiveness of activities that elicit a group affective reaction.

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Books

BOOK REVIEWS

The Social Adjustment of Children: The Bristol Social Adjustment Guides. By D. H. Stott and E. G. Sykes. Price, Test Manual $3.50; specimen set of tests and recording blanks, $0.44; set of transparent scoring templates, $1.42. Pp. (Test Manual), 46, and diagnostic materials. London, University of London, Ltd., 1958.

The "Bristol Social Adjustment Guides" "offer a method for detecting and diagnosing maladjustment, unsettledness or other emotional handicap in children of school age" (p. 5). The "Guides" consist of three rating scales to be marked by adults observing the child. The scales are as follows: The Child in School (boys' and girls' forms), the Child in Residential Care, and The Child in the Family. The scales consist of a large number of descriptive items, out of which the rater selects and underlines those that apply to the child under assessment. The rater is thus "saved the strain of composing a free verbal report." The items describe "bits of behavior" and "primary expressions of emotion unmistakable in their meaning." The underlined items are transferred to a Diagnostic Form, which tends to group the responses into certain areas of disturbance. According to the authors, this process contains "virtually no risk of a child being shown as maladjusted when it is not so" (p. 7). This claim, however, is qualified in the following way: "The validity of the record made in respect to an individual child depends upon its being observed in a situation in which stable children are happy and thriving" (p. 8).

On first blush, the "Bristol Social Adjustment Guides" seem to offer a quick, easy, and rather accurate technique, if not for understanding the dynamics of a child's disturbance, at least for selecting a potential patient from a large group of children. However, the various discussions, expositions, and arguments in the "Manual" give cause for concern over a number of issues conscientiously raised but too lightly dismissed.

The authors pride themselves on their empirical approach, yet cannot resist making theoretical statements which not only are poorly integrated with the empirical attitude but also are mere restatements of rather well-known theoretical positions. For example, their discussion of "normal responses to unfavorable situations which . . . get into a state of overreadiness" (p. 11) is simply a naïve expression of the concept of overuse of the defense mechanism. While the authors urge the reader to be "free of conceptual models" and accept the "empirical discipline [as] a counsel of perfection" (p. 15), they do not follow their own advice. Conceptual models are present in the "Manual"; only they are hidden and unrecognized.

The "Bristol Social Adjustment Guides" have limitations for the American user in that the language of the items is often unfamiliar. It would be a difficult matter to rate a child on such items as "feckless," "seems to play daft," "spivvish dress," or "has stupid moods,"—the last being perhaps as good an example of linguistic difficulty as of the clearly judgmental nature of "objective" ratings in general.

MARY ENGEL, Ph.D.

Psychoanalysis and Contemporary Thought. Edited by John D. Sutherland. Price, $3.50. Pp. 149. New York, Grove Press, Inc., 1959.

This little volume is a collection of six public lectures given as part of the celebration of Freud's Centenary by members of the British Psychoanalytical Society. They include topics of general public interest, such as Psychoanalysis and—the sense of guilt, and child care, and the teacher, and art, and philosophy, and the current economic crisis. Each essay, given by an expert in his field, is a gem worthy of perusal by professional psychiatrists and psychoanalysts.

ROY R. GRINKER, M.D.

Recent Progress in Psychiatry, Vol. 3. Edited by G. W. T. H. Fleming and A. Walk. Price, $7.50. Pp. 397. Grove Press, Inc., 795 Broadway, New York 3, 1959.

Notice should be made, even though adequate review is impossible, of the third volume of an excellent progress report for psychiatry. Of the thirteen sections, special appreciation is expressed for Ashby's report on Cybernetics, Eysenck's consideration of personality tests, and Cameron's study of Child Psychiatry. This review considers the literature from 1950 to date and is quite thorough, with adequate bibliographic references. It is highly recommended.

ROY R. GRINKER, M.D.

Right-Left Discrimination and Finger Localization. By A. L. Benton. Price, $7.00. Pp. 185. Paul B. Hoeber, Inc. (Medical Book Department of Harper & Brothers), 49 E. 33d St., New York, 1959.

This monograph is an interesting and important study of the relationship of the brain to two specific forms of behavior of the human organism. It contains not only a survey and critical analysis of the literature but also an account of the author's experience. The study of these phenomena—right-left discrimination and finger localization—goes back to Jules Badal (1888) and received a powerful stimulus from the paper of Gerstmann (1924).

Since not only incompleteness of reports but also variations in method of examination make it often difficult to correlate the numerous cases published, the author gives his own methods of examination in the introduction, then, in chapters 2, 3, and 4, the detailed data on the normative aspects of right-left discrimination and of finger localization. "Typically the 6- or 7-year-old child is able to distinguish between the right and left side of his own body but shows defective appreciation of the reversal of relations involved when he has to indicate the right and left sides of a person who is facing him." There is evidence that mental age has a decisive influence on the level of performance, but one finds specific developmental deficiency in right-left discrimination not only in mental defectives but also in brain-damaged children of adequate general intelligence, in children with disability in reading, and, rarely, in normal children and adults. Right-left discrimination is a perceptual skill which develops as a function of learning, and the evidence indicates that the integrity of the cerebrum is a necessary condition of this learning. When the brain is damaged, improvement in this skill is obtained only by long practice and remains imperfect.

The behavior known as finger localization develops at about the same rate and under the same conditions. It is somewhat associated with right-left discrimination, but specific deficit of this skill may occur. Mental level is shown again to be an independent determinant of this capacity.

The author maintains that disturbances of right-left discrimination fall into two categories: (1) impairment in response to the verbal symbols *right* and *left*, and (2) failure of lateral localization of stimuli applied to the body. These two categories of disturbance may be associated with each other but are essentially discrete. Failure to make such distinctions results in much confusion and difficulty in assessing the reports in the literature. All of these deficits may occur in relative isolation or be associated with numerous other deficits of aphasic or apraxic nature, as in the classical Gerstmann syndrome or the allochiria of Obersteiner. The author maintains that the two types of right-left discrimination which he has distinguished are related only in an indirect way, but that both types of impairment may occur as specific impairment of an underlying global deficit.

This global deficit is usually said to be a disturbance of the body schema. The development of this concept is traced by the author, who notes that it is vague and insufficient as an explanation. It is most successful in explaining such phenomena as phantom limb. The author favors a further differentiation of the body schema in the form of a right-left gradient, which can be presumed to underlie the most elementary right-left discrimination, but supposes a factor of symbolic representation to enter the picture later to account for wider application at more complicated discriminative levels. He emphasizes the need for further empirical studies in the fields of practice, sensorimotor organization, and language function, to test the explanatory value of his construct.

Because of the clinicodiagnostic implications of these studies, the problem of the location of cerebral lesions which lead to defects of these forms of behavior has attracted much attention. The author discusses the available literature and concludes with the following general formulation: "Bilateral right-left disorientation may occur as a consequence of

a lesion either in both hemispheres or in the dominant hemisphere. Unilateral right-left disorientation may occur as a consequence of a lesion in either hemisphere."

In general, the author's argumentation is sound and cautious. He is not above stretching a point occasionally, however, to make the data fit into his formulations. For example, he tries to involve the temporal lobe in the impairment of right-left discrimination by supposing it to cause disturbances of symbolic formulation. He cites in evidence a patient of Pollak who had a tumor of the left frontal lobe, which, he says, "probably caused some degree of temporal lobe dysfunction."

This monograph is an excellent discussion of the development and disturbance of these complicated behavioral phenomena and of their relationship to the brain. It should be, as its author hopes, the starting point of further observations necessary to clear up the numerous unsolved problems.

PERCIVAL BAILEY, M.D.

M.K.P.—Myokinetic Psychodiagnosis. By Prof. Emilio Mira y Lopez, with a foreword by Dr. Gordon Allport and an editor's preface. Price, $6.75. Pp. 186. Logos Press, Box 273, Cooper Station, New York 3, 1958.

In his foreword, Dr. Gordon Allport indicates that this book is important because "first it contains a diagnostic method of great promise . . . . and second it fixes our attention on the most neglected area of personality theory—the area dealing with underlying dispositions of temperament as these are manifested in expressive movements." One must agree with Dr. Allport, for, despite the fact that a number of theortical, empirical, and clinical issues are omitted, Professor Mira y Lopez has developed an exciting procedure of great promise. The M.K.P. makes available for study the area of expressive movements, for, though there have been several earlier attempts to study expressive movements, and though data from this domain are frequently used by clinicians (i.e., posture, gait, and handwriting), there has been no technique which approaches these issues with the organized and systematic methodology inherent in the M.K.P.

Mira y Lopez' postulate of psychomyokinesis is that "each intention or reaction is accompanied by a determined muscular attitude which tends to favor projected movements and inhibit non-projected ones." "The muscular groups which permit the execution of planned and wished-for actions receive an augmentation of their postural tone and, consequently, are in the condition of facilitation for their contraction, while those which oppose such an execution are found automatically inhibited in their postural tone." Thus the author makes the basic assumption that psychic activity expresses itself in somatic activity, and, particularly, in the tonicity of the skeletal muscular system. Different aspects of this muscular tonicity are studied by having subjects reproduce, in various planes (horizontal, vertical, and sagittal), and without the aid of visual cues, six sets of simple line figures. The systematic deviations of these drawings from a standard are utilized as indications of the predominant muscular group, and the subject's dominant action and psychological tendencies in the considered spatial plane.

In the interpretation of the drawings the following assumptions are made:

1. The dominant hand reveals the characteristic attitudes and reactions of the more practiced and controlled aspects of the subject. These are seen as transitory, phenotypic, and conscious.

2. The expression of the weaker hand reveals the predominant, constitutional, genotypic, instinctive expressions.

3. The degree of balance and similarity between the drawings of the two hands is the measure of intrapsychic coherence and integration.

4. Deviations in the sagittal plane relate to aggressiveness or passivity.

5. Deviations in the vertical plane relate to depression or euphoric excitation.

6. Deviations in the horizontal plane relate to intratensive or extratensive tendencies.

Other parameters obtained from the drawings are constitutional emotionality, tendency toward anxiety or apathy, excitation or inhibition, constancy of reactions, intelligence, and indications of pathological trends in the personality make-up.

This is an all-too-short introduction to an ingenious and fascinating technique. Although in large measure the book is devoted to details of administration, scoring, and normative data, the few pages that deal with theoretical rationale and the presentation of clinical findings suggest that this procedure has great promise. Many questions can be raised about the lack

of theoretical rationale, the questionable and sometimes esoteric interpretative assumptions, the brevity of the clinical summaries, and the limited and inadequate data on reliability and validity. These shortcomings, however, do not detract from the intrinsic value of the procedure or from the significance of the findings that are presented. In these findings the characteristics of schizophrenia are reported as disorientation or axial twisting, a tendency to reverse the movement being performed, a tendency toward disintegration of the configurations, irregularity in tempo of performance, and a lack of synchronization of the two hands. In the reactive form of schizophrenia, the abnormalities are usually expressed in the dominant hand. As the distortions occur in both hands, it is evidence that the disease process is more malignant. In cases which clinically seem to be in remission, if the distortions are only reduced in the dominant hand but persist in the other, a relapse is considered to be likely. The disappearance of left-hand abnormalities offers a more favorable prognosis.

In "cyclothymic states," the most striking distortions occurred in the vertical plane, and the degree of the distortion is related to the severity of the symptom. The "asthenic depressions" are reported to have no increase of aggression in the sagittal plane or of introversion in the horizontal axis. In the "dysthymic depressions," there is evidence of rage directed against the self, and, although the depressive signs persist, they are accompanied by introversion on the horizontal plane. The characteristics of the "reactive depression" manifest themselves in psychomotor-tension deficit in the dominant hand. The uneducated, left hand presents changes only in the vertical plane. Convincing data are also presented on the diagnosis of epilepsy and on the differentiation between intellectual deficit and central nervous system damage. Though it would have been helpful to have seen empirical data on these findings, and to know more about the criteria used in differentiating the types of schizophrenias and depressions, it is apparent that Professor Mira y Lopez has developed a valuable diagnostic technique.

The procedure takes on even greater significance, since it fills a gap in the types of psychodiagnostic procedures that are available. There has been a proliferation of procedures along the "perceptual-associative" modality of the Rorschach test and along the "apperceptive-dynamic" dimensions of the Thematic Apperception Test, but the "motor-expressive" area has been sorely neglected. The M.K.P. may not assume a predominant role in the usual clinical battery, for it gives little insight into the dynamic and structural aspects of personality. However, it should prove to be invaluable in situations where nonverbal diagnostic procedures are required. More important, however, is the fact that now motor correlates of nonverbal, and hopefully preverbal, experiences can be systematically studied by this technique and integrated with observations made on higher levels of psychological development. The integration of these various levels of psychological functioning should aid in refining diagnostic procedures, as well as in making significant contributions to psychological theory. The introduction of the M.K.P. in its present form should stimulate necessary research to examine the theoretical assumptions and clinical utility of the test.

SIDNEY J. BLATT, Ph.D.

Insulin Treatment in Psychiatry. Edited by Max Rinkel, M.D., and Harold E. Himwich, M.D. Price, $5.00. Pp. 386. Philosophical Library, Inc., 15 E. 40th St., New York, 1959.

This book is a symposium of the papers and discussion presented at an International Conference on Insulin Treatment in Psychiatry held at the New York Academy of Medicine on Oct. 24 and 25, 1958. The conference was sponsored, among others, by the Manfred Sakel Foundation in honor of the first anniversary of Dr. Sakel's death, the 30th anniversary of the introduction of insulin coma treatment, and to reevaluate insulin treatment in schizophrenia, in view of unfavorable recent reports, such as those by Ackner, Bourne, and Bowes.

The book is divided into three parts. The first part contains a history of the organic treatment of schizophrenia by Hans Hoff, Sakel's former associate, in which he claims that "more than 80% of the patients who have been in mental asylums are returning to their families." This is an even more miraculous claim than Sakel's original statistics and is apparently accomplished by the use of "a combination of biological treatment, such as insulin and electroshock, supplemented by the use of tranquilizers, by psychotherapy, by occupational therapy, and by psychological training of the relatives."

Dr. Joseph Wortis has an interesting chapter on the history of insulin shock treatment in which he states that insulin has been used in small doses in the treatment of psychiatric

patients since 1923 and that, although there were some improvements, it took Sakel's courage and vision to produce the shock which previous observers feared.

The second part is on physiochemical research and would be of interest to the laboratory research worker.

The third part is on "Clinical Research and Follow-Up Studies," from many parts of the world. In some areas there are active insulin units. In others, especially in the United States, a lowering of patient population in state hospitals is attributed to the use of tranquilizers. It was pointed out by some discussants that the type of remission with the tranquilizers is not as good as the one following insulin.

Dr. Bogoch reported an interesting piece of research, in which he found that neuraminic acid in the spinal fluid is at a lower level in the schizophrenic patient and remains low even after a remission following insulin coma treatment. This is reminiscent of the persistence of schizophrenic findings in the Rorschach test even after a clinical remission.

On the whole, it appears that insulin coma treatment is not being used as much as in the past; but it is a treatment that should not be forgotten in this age of drugs and should be used in those patients who cannot be reached by psychotherapy and for whom the drugs act only as an expensive sedative. If it is used at all, it should be used in coma doses. With further biochemical and pharmacological research, a drug may some day be found which will do what insulin does without the dangers of the insulin treatment. When patients have improved from severe schizophrenic symptoms, it still takes psychiatric acumen and skill to determine who should have psychotherapy or psychoanalysis, what parameters should be employed, and who should be allowed to keep his neurotic defenses, instead of risking a recurrence of his psychosis through an uncovering type of therapy.

This book is an excellent summary of organic treatment and should be read by psychiatrists who treat psychotic patients. Sakel certainly stimulated a tremendous amount of research in a condition formerly considered as almost hopeless, and this research is by no means limited to the organic but includes the psychological, sociological, and existential disciplines as well.

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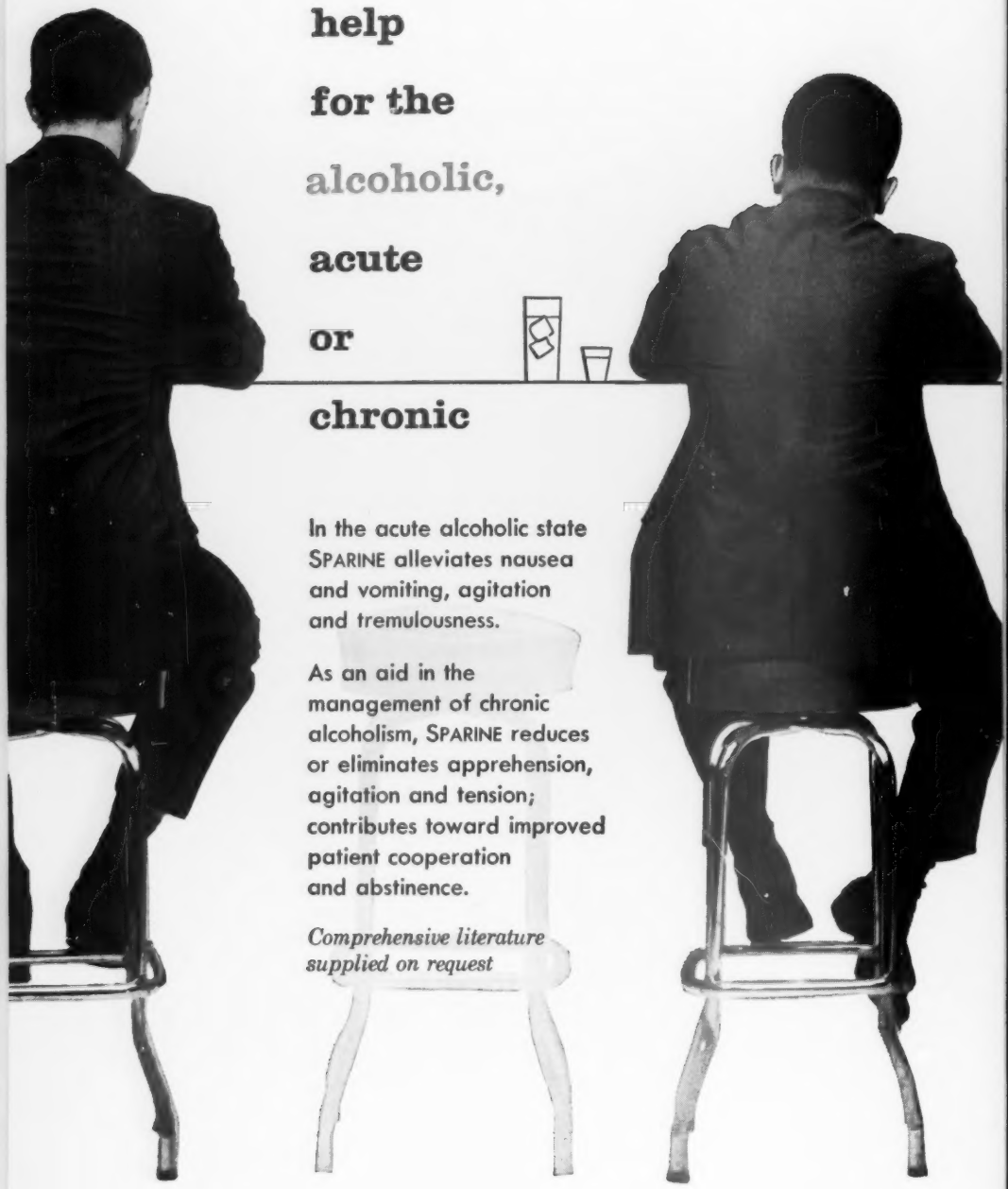

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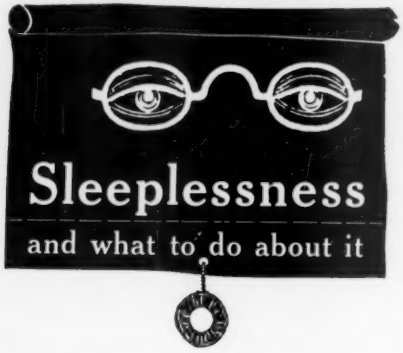

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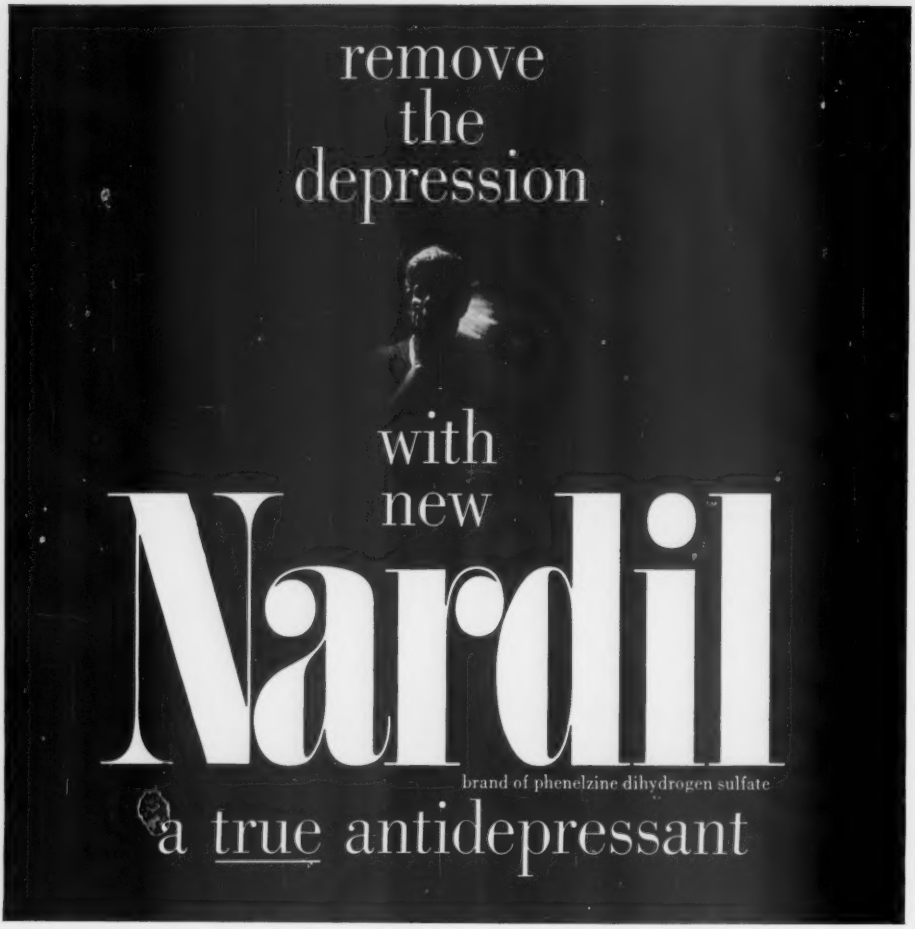
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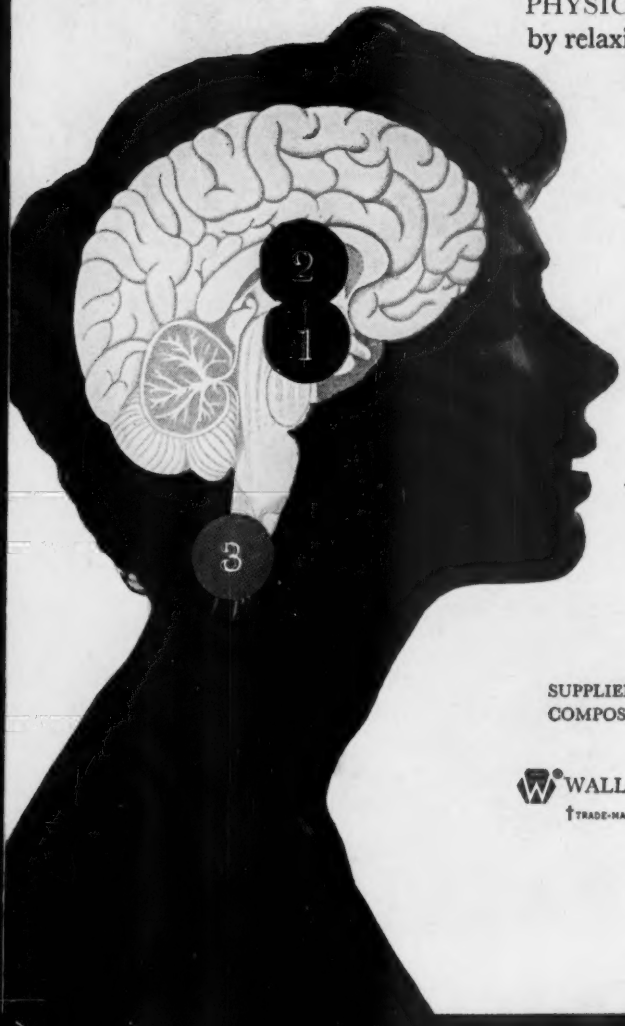

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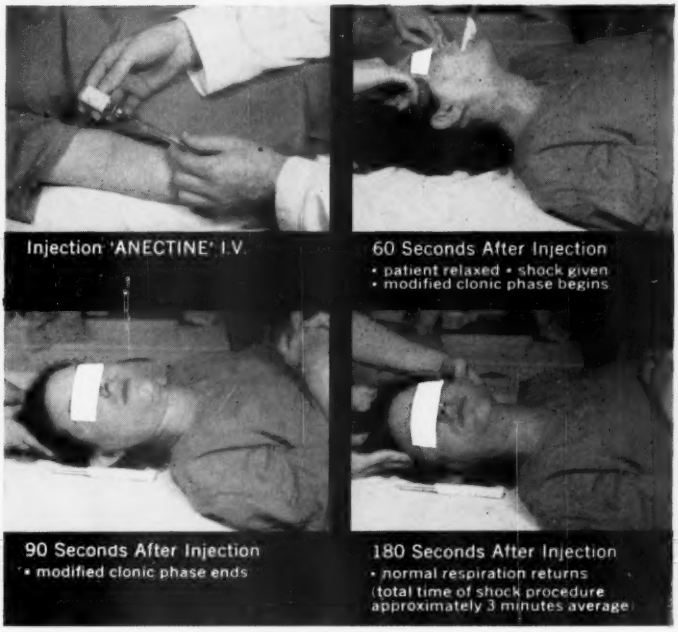
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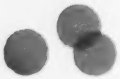

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